

The Fiscal Multiplier of Education Expenditures: Evidence from Pell Grants*

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Abstract

This paper examines the short-run effects of education expenditures on local income and employment. We estimate fiscal multipliers using cross-sectional variation in city-level exposure to the \$30-billion Federal Pell Grant Program, which is the largest U.S. program to help low-income students attend college. An increase in Pell grants by 1% of a city's income raises local income by 2.8% and local employment by 1.9% over the next two years, both exceeding estimates for military spending (1.5% on average). Multipliers are higher when grants are awarded to students at non-profit, 4-year, and high-quality colleges. Multipliers are also higher during recessions than in expansions, suggesting that Pell grants can be an effective tool for countercyclical policy that adds to long-term benefits, such as increasing the affordability of college.

Keywords: Fiscal Expenditure, Scholarships, Pell Grants, Education Policy, Fiscal Multipliers.

JEL classification: E62, E32, H52, H30, I28

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1. Introduction

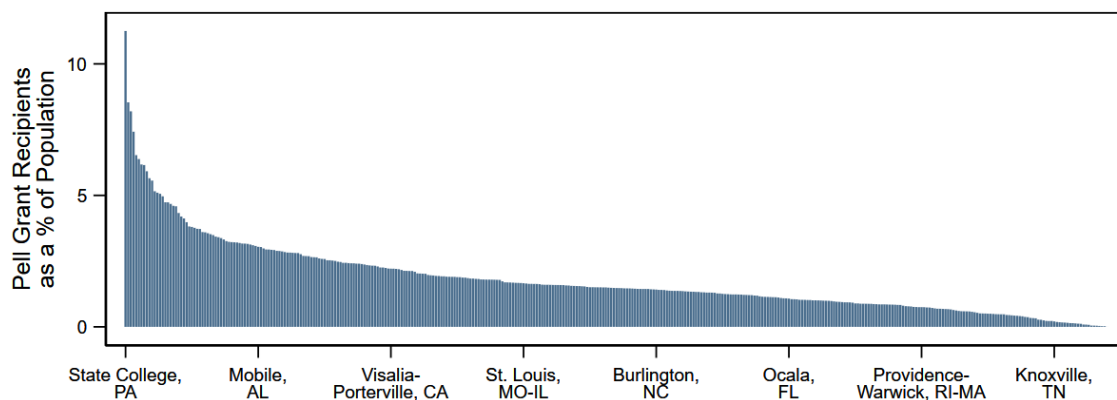
Investments in education make up a significant part of government spending in advanced economies. In the United States, educational spending measured 5.43% of national income in 2021, which exceeds defense spending and spending on welfare programs. These investments are usually motivated by the well-documented effects that education has on well-being and economic growth in the long run (see, e.g., [Barro, 1991](#), [Benhabib and Spiegel, 1994](#), [Bils and Klenow, 2000](#), and [Manuelli and Seshadri, 2014](#)). Like any other form of government spending, however, educational investments also have the potential to stimulate economic activity in the short run. Programs that reduce the cost of tuition or that involve direct transfers to students could, for example, increase purchasing power and therefore raise consumption and employment. They also unlock complementary sources of income that arise when students go to college, such as student loans. Such programs could be used to stimulate economic activity during recessions and serve as a tool for macroeconomic stabilization. Yet empirical evidence that establishes the magnitude of the short-run effects of educational investments on economic activity remains scant.

We quantify the effect of educational investments on economic growth in the short run. Specifically, we measure the impact of the Federal Pell Grant Program at the city (Metropolitan Statistical Area–MSA) level. Pell grants are need-based grants to low-income undergraduate and select post-baccalaureate students, designed to enable them to access post-secondary education. The Federal Pell Grant Program is the largest program to help low-income students attend college in the United States, with total awards exceeding 30 billion U.S. dollars in 2023. To express the effect of this program on an MSA’s economic growth, we estimate the program’s fiscal multiplier, which is the increase in income and economic activity for a given increase in the program’s spending. Our estimate of the fiscal multiplier quantifies the effect of a *relative* increase in Pell grant disbursements on the *relative* increase of a city’s aggregate income and employment. If the income from Pell grants acts as a substitute for other income, such as income from work, the multiplier of Pell grants will be between 0 and 1 for income and negative for employment. In contrast, if Pell grants enhance local economic activity, the multiplier will be above 1 for income and positive for employment.

We obtain a causal estimate of the multiplier of the Pell Grant Program using a shift-share instrumental variable approach that exploits cross-sectional variation in Pell-grant eligibility. Rather than directly relating increases in local Pell grants to local growth, we instrument local Pell grants by the interaction of national changes to Pell grant disbursements with the fraction of a city’s population that received Pell grants *prior to* the change. This exploits the fact that there exists significant variation in the share of local population that is eligible for Pell grants (Figure 1). Our strategy is an example of the “shares” approach to shift-share instrumental variables, which enables a causal identification of local fiscal multipliers using cross-sectional variation—that is valid even if national changes in Pell grants are endogenous—as long as certain conditions are met. To assess these conditions, we employ a series of validity tests proposed by [Goldsmith-Pinkham et al. \(2020\)](#) and show that our shift-share instrument passes these tests.

We find that Pell grants have a significantly positive effect on economic activity. Our main result is that the multiplier of Pell grants—the percentage increase in a city’s relative income or employment from a relative increase in Pell grants by 1% of initial income—is 2.8 for our full sample on local income and

Figure 1. The Pell Grant Program: Recipients as a Share of Population, by MSA



Notes: The figure plots the average fraction of a city's population that is eligible for Pell grants, in descending order. Data is obtained from the Title IV Program Volume Reports by the Department of Education.

1.9 for employment. This means that a dollar spent on Pell grants creates (more than) twice as much relative economic activity. This estimate is robust to the inclusion of city and time fixed effects, city-size weighting, controls for spending by state governments, other main fiscal transfers to low-income households, and various other controls for the economic performance of a city.

We then unpack the mechanism behind the multiplier in three steps: we ask whether Pell grants create a local demand impulse, how local labor markets accommodate that impulse, and which spending channels generate it. First, the employment response is concentrated almost entirely in the non-tradable sector, indicating that Pell grants raise employment by increasing local spending. Second, local labor markets absorb this demand through both lower unemployment and higher labor-force participation, and employment rises among workers aged 22 and over while falling modestly among those aged 19–21, consistent with a negative income effect on students' labor supply. Third, we use a simple accounting decomposition of the income multiplier to explain why the overall effect of Pell grants on local economic activity exceeds the size of the initial transfer. The decomposition shows that a dollar of Pell grants raises student-loan disbursements by nearly two dollars, as grants enable marginal students to attend college and borrow, and furthermore causes \$1.25 of additional institutional expenditures. The mechanical contribution of the transfer accounts for roughly a third of the multiplier, first-round spending by students and institutions for another 40 to 50%, and the residual implies local marginal propensities to consume of 0.26 to 0.39, well within the range estimated in the literature.

Our multiplier estimates come with uncertainty: our preferred estimate has a standard error of 1.5. This error is in line with multiplier estimates for other sources of spending, and our results for employment confirm that there is a significantly positive effect on economic activity when Pell grants increase. We do find that there is a high degree of variability in the magnitude of the multipliers, driven by the types of schools that Pell grant awardees attend and by the timing of the award. When comparing the effect of Pell grants that are received by students at for-profit institutions to grants for students at non-profit institutions, we find that multipliers are lower at for-profit colleges. This appears to be because for-profit schools raise tuition fees in response to an increase in Pell grant generosity, implying that Pell grants are

implicitly acting as subsidies for the for-profit university sector.¹ While we find that both for-profit and non-profit colleges raise some expenditures when Pell grants rise, we find no evidence that the majority of the positive effect of Pell grants operates through this channel. Overall, we show that the multiplier of Pell grants is highest when grants are awarded to students at high-quality, non-profit, four-year colleges. Finally, we assess whether the multiplier of Pell grants is higher during recessions, as has previously been found for military expenditure.² We find that Pell grants' economic effect is larger in recessions than in expansions, because Pell grants cause a greater increase in labor force participation during local downturns.

Our estimates of the multiplier of Pell grants add to a vast literature that uses geographic cross-sectional variation in fiscal spending to estimate its short-run economic effects. The use of geographic variation became increasingly popular in the aftermath of the Great Recession. The advantage of using geographical cross-sectional data is that there is much greater variation in spending at the sub-national level, and more of this variation is plausibly exogenous. Like other sub-national estimations of multipliers, our results do have a particular interpretation: they measure the effect of Pell grants in one city on that city's relative economic performance, rather than the effect of Pell grants on economic performance at the national level. Local multipliers are estimated under different conditions than national multipliers. First, a city-level increase in Pell grants typically does not involve an increase in city-level fiscal deficits (and subsequent taxation), such that Pell grants do not crowd out private spending. Second, a city-level increase in spending is typically sufficiently small such that it does not induce a monetary policy response. Third, local estimates may be affected by positive spillovers across local areas. [Auerbach et al. \(2020\)](#) note that local fiscal spending can have large spillovers to geographically close metropolitan areas, and find that these spillovers are typically positive.³ [Chodorow-Reich \(2019\)](#) argues, on theoretical grounds, that the kind of geographical cross-sectional multiplier we estimate remains informative. It measures the national-level multiplier of fiscal spending when it is deficit financed and when monetary policy does not respond to the fiscal expansion, for example because interest rates are constrained by the effective lower bound. As these conditions often apply during recessions, our results provide insight into the effectiveness of Pell grants as a tool to stimulate demand during downturns at the national level.

Our analysis of the multiplier for Pell grants takes into account changes in other local spending, such as unemployment insurance, housing benefits, food stamps, or state appropriations. We thus evaluate the economic impact of an increase in Pell grants while keeping other fiscal spending constant. This approach allows us to factor out the possibility that other types of spending may automatically increase or decrease in response to changes in Pell grants. Our results indicate that the effect of these automatic responses on the multiplier is minimal as controlling for other types of spending targeted to support low-income households does not change estimates significantly. This evidence suggests that the positive effects of Pell grants on economic activity are neither diluted nor intensified by shifts in other fiscal spending.

¹In general, [Turner \(2017\)](#) estimates that 11-20% of Pell grants pass through to schools.

²See, e.g., [Nakamura and Steinsson \(2014\)](#), [Auerbach and Gorodnichenko \(2012\)](#), and [Berge et al. \(2021\)](#).

³The lack of local crowding-out is consistent with the idea that there is often excess capacity in production ([Murphy, 2017](#)).

The multiplier of 2.8 for Pell grants is higher than most estimates based on cross-sectional geographic variation of other forms of government spending. Early examples include [Nakamura and Steinson \(2014\)](#), who estimate the state-level response of output to defense spending and find an average multiplier of 1.5. [Acconcia et al. \(2014\)](#) estimate multipliers from reductions in spending due to the expulsion of mafia-infiltrated city council members in Italy and find a multiplier of 1.9. Cross-sectional estimates of the multiplier were also frequently used to assess the effect of the American Recovery and Reinvestment Act (ARRA), whose timing is close to the years of the largest growth in the Pell grant program (see, e.g., [Chodorow-Reich et al., 2012](#), [Chhabra et al., 2019](#), [Conley and Dupor, 2013](#), [Dupor and Mehkari, 2016](#), [Feyrer and Sacerdote, 2011](#)) and the fiscal stimuli during the COVID-19 recession (see, e.g., [Auerbach et al., 2022](#)). [Dupor and McCrory \(2018\)](#), [Suárez Serrato and Wingender \(2016\)](#), and [Hasna \(2021\)](#) also study fiscal multipliers at a local level. [Chodorow-Reich \(2019\)](#) summarizes the literature on cross-sectional multipliers and finds that the mean estimated multiplier is 2.1 and the median estimated multiplier is 1.9. Because Pell grants are transfers to households, the most closely comparable estimates are cross-regional *transfer* multipliers. Using Social Security benefit increases and the 2001 and 2008 stimulus payments, [Pennings \(2021\)](#) estimates state-level multipliers of around 1.5 for permanent transfers but only one-third for temporary ones.⁴ This evidence suggests that the multiplier for Pell grants is high compared to the multiplier of other forms of government spending and is, therefore, an effective tool to stimulate short-run economic activity. Furthermore, we also contribute to the literature on state dependence of fiscal multipliers by examining the differential effects across expansions and recessions (see, e.g., [Auerbach and Gorodnichenko, 2012](#), [Berge et al., 2021](#), [Barnichon et al., 2021](#), and [Ghassibe and Zanetti, 2022](#)).

Why is the average effect of Pell grants on local income so large? There are three likely drivers. First, Pell grants are a direct cash transfer and thus a part of personal income. Transfers therefore raise personal income one-for-one to begin with, whereas other forms of government spending, such as infrastructure or defense, only raise local income indirectly. In our decomposition of the multiplier, this mechanical effect accounts for roughly a third of the total. Second, Pell grants are awarded to students from lower-income families, who have limited borrowing capacity and are likely to have high marginal propensities to consume (MPC). Fiscal stimuli that target high-MPC households generate higher multipliers (see, e.g., [Johnson et al., 2006](#), [Parker et al., 2013](#), [Jappelli and Pistaferri, 2014](#), and [Hausman, 2016](#)). Third, Pell grants enable many students to attend college ([Dynarski, 2003](#)) but do not fully cover its cost, leading students to rely on complementary financing sources. Consistent with this, we find that each dollar of Pell grants raises student-loan disbursements by nearly two dollars, further relaxing recipients' budget constraints. Together, these factors explain why Pell grants generate local income growth that significantly exceeds the initial transfer and multipliers above those estimated for other types of government spending.

In addition to providing evidence on the magnitude of the fiscal multiplier, this paper contributes to the literature on the Pell Grant Program. Previous work has documented several other positive effects, in

⁴Transfer multipliers of a similar magnitude to ours were previously documented in the literature: [Corbi et al. \(2019\)](#) estimate a local income multiplier of about 2 for federal transfers to Brazilian municipalities, and [Egger et al. \(2022\)](#) a transfer multiplier of 2.4 from randomized cash transfers in Kenyan villages.

particular in relation to education outcomes. [Bettinger \(2004\)](#) shows that receiving a Pell grant reduces college dropout behavior. Pell grants also increase educational attainment, the probability of attending college, credit accumulation, and have positive effects on students' persistence and degree completion ([Dynarski, 2003](#), [Castleman and Long, 2016](#), and [Fack and Grenet, 2015](#)). [Denning et al. \(2019\)](#) show that eligibility for an additional Pell grant significantly increases the likelihood of college completion and raises earnings four years after the receipt of the degree. [Dinerstein et al. \(2014\)](#) do look at the short-term benefits of Pell grants as part of various federal transfers to post-secondary education during the Global Financial Crisis. Our analysis differs from [Dinerstein et al.](#)'s because we estimate the Pell grant multiplier by exploiting the cross-sectional variation in the share of Pell grant recipients across cities. This means that our estimate of the multiplier is causal if these local exposure shares are exogenous—something we carefully assess in line with the recommendations by [Goldsmith-Pinkham et al. \(2020\)](#)—even if national changes in Pell grants are endogenous to national macroeconomic conditions. This new shift-share strategy also enables us to control for state- and time-fixed effects. [Dinerstein et al.](#) find that counties that benefited from increases in the generosity of the Pell Grant Program did not have a significant increase in local income during the Global Financial Crisis.⁵ This result, however, is based on a limited sample of counties that only covers 2007 to 2009. When their shift-share methodology is applied to our full sample, we find multiplier estimates that are consistent with ours.

Outline The remainder of this paper proceeds as follows. We start by discussing the Pell Grant Program and the empirical strategy in Section 2. In Section 3 we present our main multiplier estimates and validate the identification strategy. Section 4 examines the mechanisms behind the multiplier, while Section 5 assesses how multipliers vary across types of colleges and over the business cycle. Section 6 concludes.

2. Empirical Approach

This section outlines the empirical strategy to estimate the short-term economic effects of Pell grants. We start with a brief summary of the Pell grant program and how grants are allocated to students in Section 2.1. Section 2.2 outlines the identification strategy while Section 2.3 summarizes the dataset.

2.1. Pell Grants: Background

The Federal Pell Grant Program was initiated in 1974 as the Basic Educational Opportunity Grant to provide a need-based grant to enable low-income students to attend college. It was renamed the Pell Grant Program after Senator Claiborne Pell in 1980. It started off with 280,000 students in 1974 with a total appropriation of \$122 million, which increased to over 9 million recipients and a \$30 billion appropriation by 2023. The program expanded particularly rapidly from the early 2000s to 2010, as the U.S. witnessed

⁵We perform the analysis at the MSA rather than the county level because evidence suggests that MSAs better capture spending patterns. Using credit card data, [Dunn and Gholizadeh \(2023\)](#) show that consumers regularly consume outside their home county and that this consumption link across counties has important implications for economic measurement. In addition, [Black et al. \(2020\)](#) show that increased access to student loans increases college attainment and implies that these students do not have to rely on other sources of funding and do not have to work as much while in college.

a substantial increase in enrollment in post-secondary institutions and a marked increase in college tuition. Federal support for higher education was expanded to compensate for the increase in costs, as part of the College Cost Reduction and Access Act of 2007 and of the American Recovery and Reinvestment Act of 2009. A full summary of legislative changes is found in Appendix A.

The size of individual grants depends on a student's family earnings. The largest share of Pell grant disbursements is received by students from families with an adjusted gross income of less than \$60,000. Grant amounts are conditional on the student's expected family contribution (EFC), the student's enrollment status, and whether or not they attend a full academic year or less.⁶ A full-time student is eligible for the following Pell grant award if the maximum Pell grant ($Pell^{MAX}$) is higher than the EFC:

$$Pell_{i,t} = \max \{ (Pell_t^{MAX} - EFC_{i,t}), Pell_t^{MIN} \}, \quad (1)$$

where $Pell^{MIN}$ is the minimum Pell grant.⁷ Once the grant amount is determined, the institution at which the student is enrolled either credits the grant funds to the student's account, pays the student directly by check, or combines these methods. Grant recipients can enroll in various types of institutions, ranging from four-year colleges to those specialized in occupational training. Currently, about 5,000 post-secondary institutions participate in the program and more than 40% of all undergraduates rely on this type of aid. A significant share of grant recipients are enrolled in public two-year schools and at for-profit institutions. Pell grants do not typically cover the entire cost of attendance and, as a result, most recipients supplement this type of aid with funds from other sources, such as federal and/or private student loans, personal savings, and 529 plan savings.

2.2. Strategy

We estimate the effect of Pell grants on short-run economic activity by exploiting variation in Pell grant disbursements across cities. By relying on regional variation, we enable causal identification of the Pell grants' multiplier. At the national level, changes in Pell grants are highly endogenous to economic fluctuations. Enrollment in higher education is counter-cyclical, tending to increase when economic performance is poor, for example, inducing an endogenous negative relationship between growth and the size of the Pell Grant Program. In fact, the 2009 increase in Pell grants as part of the American Recovery and Reinvestment Act was expressly in response to poor economic performance during the Global Financial Crisis. This places a downward bias on national-level multiplier estimates.

We overcome this national-level limitation by analyzing the effect of an increase in generosity of the Pell Grant Program at the city (Metropolitan Statistical Area–MSA) level. While the generosity and conditionality of Pell grants are determined federally, there is significant variation in the extent to which sub-national areas benefit from an increase in national-level Pell grant awards. This variation is driven by the fact that areas differ in the number of eligible students in post-secondary education. A city with

⁶The Department of Education uses a standard formula to determine the EFC. The formula relies on the student's income (and assets for independent students), the parents' income and assets (for dependent students), the family's household size, and the number of family members (excluding parents) attending post-secondary education.

⁷Part-time student awards are scaled by a factor of 0.5. Part-year students receive a prorated Pell grant.

a large number of universities benefits more from an increase than a city without universities, while a city where a small fraction of its student population is eligible (e.g., because of average income) benefits less than a city where a greater fraction is eligible, even if both cities have a similar number of students overall. Metropolitan areas are the appropriate level of analysis because the vast majority of U.S. college students reside near their school.⁸ College students also tend to spend most of their income on basic household goods, such as groceries, housing, transportation, and health care, and spending for these categories mostly occurs within the MSA the school is located in.

The estimation equation that we use to estimate the MSA-level Pell multiplier reads as follows:

$$\frac{\Delta y_{m,t;t-2}}{y_{m,t-2}} = \beta \left(\frac{\Delta e_{m,t;t-2}}{y_{m,t-2}} \right) + \phi_m + \psi_t + \gamma' x_{m,t-2} + \mu_{m,t}, \quad (2)$$

where β is the multiplier, $y_{m,t}$ is per-capita personal income in metropolitan area m in year t , while $\Delta y_{m,t;t-2}$ is its biannual change. $\Delta e_{m,t;t-2}$ is the biannual change in the per-capita transfer of Pell grants to students enrolled in schools in m . It follows that our estimates of the multiplier of the Federal Pell Grant Program, coefficient β in eq. (2), measures the *relative* increase in metropolitan area m 's income when it achieves a relative increase in Pell grants of 1% of local income. $x_{m,t-2}$ is a vector of local control variables, while ϕ_m and ψ_t denote fixed effects for metropolitan areas and years, respectively. We measure economic activity through personal income, which is a measure that is highly correlated with GDP.⁹ We use biannual changes to mitigate the noise coming from the mismatch between calendar years and academic years, and to account for the fact that shocks to spending tend to materialize in the second year. When estimating the effect of Pell grants on employment, we replace the dependent variable by $\Delta l_{m,t;t-2}/l_{m,t-2}$, where $l_{m,t}$ is local employment.¹⁰

To obtain a causal estimate of Pell grants' multiplier β , we must still address the possibility of endogeneity in changes to local Pell grant awards. Increases in Pell grant awards at the level of a metropolitan area may respond, for example, to an increase in local college enrollment that is driven by a deterioration of local economic conditions. This again puts a downward bias on the multiplier estimates.

We address this identification problem using shift-share instrumental variables (SSIV). We rely on the “shares-approach” identification strategy, as proposed by Goldsmith-Pinkham et al. (2020). In particular, we construct a shift-share instrument $b_{m,t}$ that equals the interaction of the national growth in Pell grant disbursements and the fraction of a metropolitan area's population that received a grant two years prior:

$$b_{m,t} = \left(\frac{\Delta e_{t;t-2}}{y_{t-2}} \right) \cdot s_{m,t-2} \quad (3)$$

⁸According to the 2015 Digest of Education Statistics Table 309.10 covering student residence and migration, 82% of first-time degree-seeking undergraduate students attend college within their state of residence. Dunn and Gholizadeh (2023) show that consumers regularly consume outside their home county, so areas such as MSAs or commuting zones may be more appropriate for the analysis of fiscal multipliers than counties. Additionally, there is more variation in spending across metropolitan areas than at other levels commonly used in the estimation of multipliers, like at the state level. Over the complete sample, the ratio of Pell grant spending to GDP is 0.16% across MSAs with a standard deviation of 0.17%, while that ratio is 0.12% at the state level with a standard deviation of just 0.08%.

⁹MSA GDP is only available from 2001. The correlation between GDP and personal income is 0.997 in overlapping years.

¹⁰To keep definitions consistent, we express employment in per capita terms as well, i.e, we study local employment to population ratios.

where $\Delta e_{t;t-2}$ is the bi-annual change in national Pell grants while y_{t-2} denotes the twice-lagged national mean of personal income, and where $s_{m,t-2}$ denotes the share of a city’s population that received a Pell grant two years prior. Our SSIV therefore leverages variation in the density of Pell grant recipients across cities prior to an increase in the program’s generosity to identify the grants’ effect on short-term growth.

Goldsmith-Pinkham et al. (2020) show that an SSIV along the lines of the one described in eq. (3) enables a causal identification of β as long as (i) the instrument $b_{m,t}$ is relevant and (ii) the “shares” in the SSIV are orthogonal to the structural error term. The first condition is straightforward to verify by regressing the instrument on changes in a city’s Pell grant receipts. The second condition, which in our case requires that the shares $s_{m,t-2}$ be orthogonal to $\mu_{m,t}$ in eq. (2), cannot be verified directly. Instead, we conduct a series of falsification tests recommended by recent papers on the use of SSIVs (Goldsmith-Pinkham et al., 2020, Borusyak et al., 2022) to validate the strategy and show that our SSIV passes these tests consistently.

2.3. Data

To estimate the short-run economic effects of the Pell Grant Program we analyze a sample of 366 metropolitan areas with data from 1990 to 2015.¹¹ Summary statistics are provided in Table 1. We obtain data on personal income from the Bureau of Economic Analysis (BEA) and our data on employment from the Bureau of Labor Statistics (BLS). Our data on the Pell Grant Program comes from two sources: the Delta Cost Project—an independent, nonprofit organization that provides estimates based on data from the Integrated Postsecondary Education Data Systems—and the Title IV Program Volume reports published by the Department of Education. These datasets provide information about Pell grant disbursements at the level of higher education institutions. We aggregate the data to the metropolitan area level, which we are able to do for around 87% of Pell grants.¹² Delta Cost is our primary source for Pell grant data because it covers the entire sample period; the Department of Education’s data is available only from 2000.¹³

Our control variables come from a variety of sources. We obtain from the Delta Cost and Department of Education datasets various characteristics of an MSA’s higher education institutions. These include the number of undergraduate students enrolled and the average tuition fee they pay, information on the fraction of institutions that is for-profit, and whether institutions primarily offer two- or four-year degrees. We additionally obtain financial control variables from Equifax credit bureau data through the

¹¹We start from the universe of MSAs and exclude areas that never receive Pell grants and MSA-years with bi-annual income changes of less than -5%. This ensures that we do not include local “natural” disasters in the sample. If we were to include these natural disasters, the estimates of the multipliers would remain roughly the same; if anything they would increase slightly.

¹²The remainder of Pell grants is awarded to rural areas. Multipliers estimated at the county level confirm our main results.

¹³A small fraction of observations in Delta Cost is adjusted or imputed. To validate the Delta Cost data, we compare the MSA-level Delta Cost data with the available DoEd data aggregated at the same level. This comparison reveals 16 areas where Pell grants from Delta Cost differ erratically from the DoEd data, which we address in two ways on a case-by-case basis. First, for the cases when one year of data was missing or one MSA-year observation was considered suspicious, we used linear interpolation based on the Delta Cost data. Second, for the cases when multiple MSA-year observations were either missing or were questionable, we applied the growth rate observed in the DoEd data to Delta Cost data. From our sample of 366 MSAs we correct the path of Pell grants for 9 using interpolation and 10 using the DoEd growth rate. Given that Pell grant recipient data is not available between 1990 and 1999, we impute the Pell grant recipient share ($s_{m,t}$) for these years. To do so, we approximate the number of Pell grant recipients in an MSA by dividing an MSA’s Pell grant disbursement by the maximum per-capita Pell grant that year, as most students receive the maximum amount. The correlation between the actual Pell grant recipient shares and the imputed shares is 0.925. We always include specifications that only rely on data from 2000 to 2015.

Federal Reserve Bank of New York’s Consumer Credit Panel. We use this dataset to control for student and overall debt, median Equifax Risk Score, mortgage delinquency, and credit card utilization. Data is available for the post-1999 period at quarterly frequency, which we annualize by taking averages. We also retrieve demographic control variables for race and average education levels from the Census Bureau. Lastly, we classify the quality of higher education institutions using the college quality index of Carruthers and Welch (2019), which averages five standardized measures—25th and 75th percentile ACT intake, instructional expenditures per FTE, the faculty-student ratio, and the graduation rate—and we define high-quality schools as those scoring above the sample median.

We use Delta Cost to obtain data on state appropriations for higher education. As we aim to measure the fiscal multiplier effects of Pell grants, we control in some specifications for state appropriations as they may be substitutes or complements for Pell grants. This means that without controlling for appropriations, our estimated multipliers would not measure the effect holding other fiscal spending on higher education constant. For readers interested in understanding how the Pell grants’ multiplier may be amplified or dampened by changes in other spending, we also present results without this control.

We also use data on the main U.S. fiscal transfer programs for supporting low-income households to control for possible confounding shocks. Data on the Supplemental Nutrition Assistance Program (SNAP) comes from the USDA and is available at the county level for most states for a single month in the year (July). We then multiply SNAP disbursements by 12 to get yearly amounts. Data on housing assistance to low-income households comes from the U.S. Department of Housing and Urban Development (HUD) which is available at the MSA level after 2004. HUD data provide average government spending per unit per month which we multiply by the total number of units and by 12 to get yearly spending. Finally, unemployment insurance (UI) spending data comes from the Department of Labor. This data is only available at the state level. To get MSA-level estimates, we assume MSA disbursements are proportional to the MSA’s share of unemployed calculated at the state level. For MSAs that cross state lines, we further assume that their population is split between states proportionally to their population levels. Finally, we replace missing data with the respective yearly sample average for each fiscal transfer program.

3. Main Results

This section presents the estimates of the fiscal multiplier of Pell grants. Section 3.1 presents the main estimates, while Section 3.2 presents a series of validation exercises on the identification assumption.

3.1. Multiplier Estimates

The main multiplier estimates are presented in Table 2. The upper panel shows the effect of Pell grants on local income per capita while the lower panel shows the effect on employment. Columns (1) to (4) use data for all years while columns (5) to (10) focus on the post-1999 sample for which we have additional controls. The estimated coefficient, which is β from equation (2), represents the multiplier.

Table 2 shows that the Pell Grant Program has an economically and statistically significant multiplier effect. In the simplest specification for the full sample, we find a multiplier of 2.7, which means that an

Table 1: Summary Statistics

	Mean	St. Dev.	Obs	Min	Max	Source
<i>Dependent Variable</i>						
Δ Personal Income (Biannual)	0.036	0.038	8,436	-0.050	0.624	BEA
Δ Employment (Biannual)	0.007	0.028	8,436	-0.130	0.251	BLS
<i>Pell Grants and SSIV</i>						
Growth in Expenditure - MSA	0.016	0.067	8,436	-0.757	1.084	Delta Cost
Growth in Expenditure - National	0.012	0.031	8,436	-0.029	0.116	Delta Cost
Pell Recipients Population Share	1.743	1.691	8,436	0.000	22.681	DoE
Appropriations Share of Income	1.005	1.701	8,436	0.000	15.210	Delta Cost
<i>Loan Disbursements</i>						
Growth in Loan Disbursements	0.062	0.221	4,447	-2.544	3.237	Delta Cost
<i>Control Variables</i>						
Growth in Appropriations	-0.012	0.237	8,436	-5.237	6.664	Delta Cost
Students (log change)	0.028	0.152	8,436	-4.222	4.352	Delta Cost
Tuition fee (log)	8.606	0.835	8,436	4.745	10.768	Delta Cost
For Profit (%)	18.873	20.148	8,436	0.000	81.818	Delta Cost
Black (% of Population)	10.414	10.668	8,436	0.094	52.672	Census
Hispanic (% of Population)	9.661	14.142	8,436	0.284	95.745	Census
Bachelor's Degree (% of Pop.)	9.342	2.646	8,436	3.200	21.018	Census
Credit Card Utilization Rate	26.744	6.033	4,447	8.004	65.020	CCP (post 1999)
Age (Median)	47.549	4.385	4,447	27.500	63.000	CCP (post 1999)
Risk Score (Median)	701.264	34.814	4,447	583.875	787.188	CCP (post 1999)
Mortgage Delinq. (%)	5.713	4.803	4,447	-7.601	64.438	CCP (post 1999)
Total Debt (Ex-Student Debt, % of Income)	4.002	3.331	4,447	0.043	42.245	CCP (post 1999)
Student Debt (% of Income)	0.278	0.329	4,447	0.001	4.812	CCP (post 1999)
SNAP Share of Income (%)	0.459	0.288	4,447	0.000	3.346	USDA
UI Share of Income (%)	0.361	0.210	4,447	0.030	1.616	DoL
HUD Share of Income (%)	0.200	0.104	4,447	0.001	0.799	HUD
Growth in Fiscal Transfers (%)	0.018	0.381	4,447	-5.221	3.571	USDA, DoL, HUD
<i>Additional Variables</i>						
Population (log)	12.604	1.066	8,436	10.674	16.820	NBER
Unemployment (% of Population)	3.551	1.500	8,388	0.860	20.290	BLS
Not in the Labor Force (% of Population)	39.529	7.610	8,388	10.582	65.931	BLS
Growth in Net Tuition (%)	0.068	0.241	7,762	-5.313	5.244	Delta Cost
Growth in Institutional Spending (%)	0.082	0.333	8,436	-7.045	8.298	Delta Cost
Total Postbaccalaureate (% of Population)	0.640	1.197	7,778	0.000	11.246	Delta Cost
Employment (Stable, % of Population)	29.996	7.406	6,561	0.176	54.692	QWI
High-Quality Schools (%)	46.983	31.895	7,647	0.000	100.000	Carruthers and Welch (2019)

Notes: Summary statistics for the merged sample. Data from 1990 to 2015 covering 366 metropolitan areas. BEA stands for Bureau of Economic Analysis. BLS: Bureau of Labor Statistics. CCP: Federal Reserve Bank of New York/Equifax Consumer Credit Panel. DoE: Department of Education. USDA stands for US Department of Agriculture. DoL: Department of Labor. HUD: Department of Housing and Urban Development. QWI: Quarterly Workforce Indicators from the Census.

increase in Pell grant disbursements of 1% of local income raises local income by 2.7% relative to other areas. This specification controls for MSA-level and time fixed effects, as we do across all columns.¹⁴

¹⁴Appendix Table A7 presents results where we check whether city size matters for the estimated multiplier (Shoag, 2013), weighting observations by the two-year lagged log of MSA population. The estimates are very similar to those in Table 2. Table A8 further shows that results are also robust to using the change in maximum Pell grants as the “shift” variable in the shift-share instrumental variable strategy, which is the shift variable used by Dinerstein et al. (2014). We have also considered specifications where we control for the interaction of state-fixed effects and year fixed effects instead of MSA and time fixed effects. This allows for arbitrarily varying time trends at the state level. Doing so yields nearly identical multiplier estimates.

In subsequent columns, we add increasingly stringent controls. We first add changes in state appropriations for higher education.¹⁵ Previous work has shown that state appropriations often respond to changes in Pell grants. During the 2009 recession, for example, state appropriations fell by 29 cents on average per dollar increase in federal funds to colleges, while some states even reduced appropriations proportionally to increases in Pell grants (Dinerstein et al., 2014).¹⁶ In our data, controlling for such alternative spending does not meaningfully alter the multiplier effect of Pell grants. We then add a series of additional MSA-level controls in column (3): biannual change in the log of undergraduate student count, average tuition and the percentage of schools that is for-profit, demographic controls consisting of the percentage of the population that is black, Hispanic, and the fraction that has at least a bachelor's degree. Doing so raises the multiplier of Pell grants to 2.8. As this is our richest specification for the full sample using a 2SLS estimator, 2.8 is our preferred estimate. Adding a lagged dependent variable (column 4, with a system-GMM estimator to address Nickell bias) again has a limited effect on the size of the multiplier.¹⁷

In the post-1999 data we find multipliers up to 3.6. This increase in multipliers is likely because recession years make up a larger share of the shorter sample, which means there are more idle resources that can respond to a Pell-grant induced increase in demand (see Section 5). In column (8) we add the controls from the New York Federal Reserve Bank/Equifax Consumer Credit Panel data: credit card utilization rates, age, Equifax Risk Score, mortgage delinquency, and total debt as a percentage of income excluding student debt. Adding these controls yields a multiplier of 3.0. In column (9) we reproduce the specification of column (8) using the broader definition of changes in fiscal spending including state appropriations as well as SNAP, HUD, and UI, and thus capture all the major fiscal transfers to low-income households. Our estimate of the Pell grant multiplier does not fall when including these fiscal spending controls. Rather, there is a 0.19 point increase in the multiplier when controlling for the broader fiscal spending, which is well within a standard error. Thus, the Pell grant multiplier is not driven by other fiscal spending that is positively correlated with exposure to Pell Grant disbursements. The system GMM estimator in column (10) gives a multiplier of 2.7, close to our preferred estimate of 2.8 in column (3).

The bottom panel of Table 2 repeats these specifications with the biannual change in the employment rate as the dependent variable. This means that the multiplier measures the percentage by which the local employment rate increases relative to other areas when Pell grant disbursements increase by 1% of local personal income. Estimates of the fiscal multiplier on employment suggest that the employment fiscal multipliers are between 1.6 and 3.2, with the preferred estimate around 1.9 for the full sample. This

¹⁵As state appropriations might also be endogenous, we instrument it with a SSIV that is analogous to the one we use for Pell grants, i.e., we use national changes in appropriations spending and the twice lagged appropriations share of local income.

¹⁶Reductions in state appropriations tend to have negative effects on students. Webber (2017) shows that for every \$1,000 per student state budget cut, the average student pays \$257 more in tuition and fees. Goodman and Volz (2020) find that changes in appropriations induce students to substitute between public and for-profit colleges and affects student borrowing.

¹⁷To choose the GMM instrument specification, we follow a simple algorithm for each regression. First, we determine the lowest lag for each model specification (e.g., difference) for which the regression passes the standard AR and over-identification tests, and consider these the candidate instrument specifications. Second, we compare the first-stage F-statistic across these candidates, and choose the model that delivers the highest F-statistic. The multiplier for that model is presented in the table.

Table 2: Effect of Pell Grants on Local Income Per Capita and Employment

	Full Sample				Post 1999						Full Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	2SLS	2SLS	2SLS	GMM	2SLS	2SLS	2SLS	2SLS	2SLS	GMM	OLS
Income Growth Multiplier	2.735*	2.656*	2.796*	2.604*	3.640**	3.615**	3.058*	2.967*	3.155*	2.714*	-1.672*
	(1.441)	(1.486)	(1.486)	(1.397)	(1.691)	(1.725)	(1.672)	(1.679)	(1.638)	(1.567)	(0.925)
Employment Growth Multiplier	1.620	1.835	1.930*	2.407*	3.120**	3.243**	2.855**	2.771**	2.636**	1.945	-1.559**
	(1.123)	(1.132)	(1.143)	(1.291)	(1.312)	(1.322)	(1.277)	(1.264)	(1.267)	(1.223)	(0.772)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Approp.		Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes
MSA Controls			Yes	Yes			Yes	Yes	Yes	Yes	Yes
Financial Controls								Yes	Yes	Yes	
Fiscal Transfers									Yes	Yes	
Δ Pell Grants F-test	107.9	100.0	100.0	61.5	78.0	74.2	73.7	73.5	81.3	48.7	-
Joint F-test	-	67.3	67.7	71.0	-	37.8	37.5	37.4	52.6	58.4	-
Observations	8,436	8,436	8,436	8,436	4,447	4,447	4,447	4,447	4,447	4,447	8,436

Notes: 2SLS estimates use the SSIV strategy described in eq. (3). SSIV strategies for state appropriations and wider fiscal transfers are analogous. MSA controls: change in undergraduate students (log) in the last 2 years, average tuition fee (log), for-profit penetration, percentage of population black, percentage Hispanic, percentage with at least a bachelor's degree. Data on financial controls is from Federal Reserve Bank of New York/Equifax Consumer Credit Panel (available after 1999): median Equifax Risk Score and age, debt-to-income ratio, credit card utilization, and 30-day mortgage delinquency rate. Fiscal Transfers: sum of transfers due to state appropriations, SNAP, UI, and HUD programs. Δ Pell Grants F-test is the robust F-statistic of the first-stage regression of Pell grants. Joint F-test is the robust F-statistic of the joint IV set. The GMM columns' Joint F-test is the auxiliary robust first-stage F-statistic for fiscal transfers. The [Montiel Olea and Pflueger \(2013\)](#) test has a critical value of 23.1 for a 10% worst case bias (the usual threshold), which is comfortably exceeded in all columns. MSA-clustered standard errors are in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

implies that the cost of creating a job through Pell grants is around \$30,500.¹⁸ The effect of adding control variables to the estimates for the employment multiplier is similar to the effect for income.

Taken together, the estimates in Table 2 show that Pell grants have a large short-run effect on local economic activity. The Pell income multiplier exceeds the mean (2.1) and median (1.9) of the cross-sectional estimates surveyed by [Chodorow-Reich \(2019\)](#), the military-spending estimates in [Nakamura and Steinsson \(2014\)](#), and the permanent-transfer multiplier in [Pennings \(2021\)](#). We should, however, take into account estimate uncertainty when making comparisons. If we assume sample independence, we can compare our preferred estimate (Column (3) in Table 2) with those in [Nakamura and Steinsson \(2014\)](#) and [Pennings \(2021\)](#) through one-sided t-tests. These yield p-values between 0.11 and 0.28 for various specifications in these papers. Therefore, we cannot reject that our preferred Pell grant multiplier equals the multipliers in these studies at 10% significance levels. The implied cost of about \$30,500 per job lies at the low end of the range for other federal programs ([Suárez Serrato and Wingender, 2016](#); [Auerbach et al., 2022](#)), but it is close to the roughly \$20,000 per job-year that [Adelino et al. \(2017\)](#) find for debt-financed municipal spending. The standard errors are sizable, however, and Section 5 below shows that the extent to which Pell grants' multiplier exceeds other estimates depends on the timing of the spending and the type of institution that awardees attend.

¹⁸The effect of Pell grants on employment count is given by $\partial l_t / \partial e_t = \hat{\beta} \cdot l_{t-2} / y_{t-2}$. Inserting the inverse of average personal income per employee in the sample and $\hat{\beta} = 1.93$ gives 0.328 jobs per \$10,000.

3.2. Validity Tests

We next validate our shift-share instrumental variable strategy. As our estimates rely on the “shares approach”, the identification assumption is that MSAs with a large share of Pell recipients would have experienced similar economic growth as MSAs with low shares in the absence of changes in aggregate Pell grant disbursements. As with all instruments, this exclusion restriction cannot be tested directly. Instead, we follow [Goldsmith-Pinkham et al. \(2020\)](#) and conduct a series of validation tests, including a formal balance test; an assessment, using Rotemberg weights, of which sample years drive our estimate of the multiplier; and multiple other tests that are summarized here while details are provided in [Appendix B](#).

3.2.1. Balance Tests

We start by performing a balance test of the covariates. The test consists of two regressions, one with either growth in personal income or employment growth as the dependent variable, and one with the instrument shares—Pell grant recipients as a share of the population—as the dependent variable. In each regression, all the control variables of the main analysis are the regressors. The idea behind this exercise is to assess whether there is a simultaneous correlation between *observables*, recipient shares, and income growth. If so, we might worry about omitted variable bias from *unobservables* (see, e.g., [Oster, 2019](#)).

Table 3 presents the test. The left-hand panel presents results for the full sample, while the right-hand panel includes the additional covariates for the shorter time sample. All variables are demeaned and normalized to have unit variance to assist interpretation. The test fails if one or more of the outcome variables—growth in personal income or employment growth—and the shares variable—Pell recipients as a share of the population—significantly correlate with an observable. The test passes (✓) if there are no simultaneous significant correlations for shares and either dependent variable.¹⁹

All covariates pass the balance test: no variables correlate with both the shift-share instruments’ shares and one of the outcome variables at conventional significance levels. This makes it less likely that an unobserved confounder would correlate with both income growth (or employment growth) and the ex-ante population share receiving Pell grants.²⁰ As the control variables for other fiscal expenditures are also instrumented with a shift-share instrumental variable, [Online Appendix B](#) provides similar balance checks for those instruments (Tables A2 and A3). The balance test for state appropriations passes for all but three control variables: the percentage of Hispanics and bachelor’s degree holders, and the share of for-profit colleges, while the balance test for the combined fiscal transfers also shows a correlation for the debt-to-income ratio. While optimally these balance tests would have all passed, the correlations are modest. In line with this, including appropriations has a negligible effect on the multipliers in [Table 2](#).

¹⁹[Donaldson \(2025\)](#) shows that a further relevant check is whether our shares variable—the share of the local population receiving Pell grants—is correlated with MSAs’ differential exposure to general-equilibrium variables, with monetary policy as the leading example. [Donaldson \(2025\)](#) proposes a test for such heterogeneous exposure to dynamic general equilibrium effects (HEDGE). We replicate this exercise by testing whether high Pell-recipient areas respond differentially to monetary policy, or whether monetary policy responds to Pell grants. We find no evidence of either, which means that the HEDGE test is passed.

²⁰Table A1 in [Online Appendix B](#) replaces appropriations growth with growth of the broader fiscal transfers that are part of appropriations, SNAP, HUD, and UI. That table, again, confirms that the balance test passes for all covariates.

Table 3: Balance Test for Pell Grants' Shift-Share Instrumental Variable

	Full Sample ($N = 8,436$)				Post 1999 ($N = 4,447$)			
	IV Shares	Income Growth	Empl. Growth		IV Shares	Income Growth	Empl. Growth	
Appropriations Growth	0.003 (0.014)	0.032*** (0.010)	0.051*** (0.014)	✓	0.001 (0.008)	0.033*** (0.011)	0.047*** (0.013)	✓
Log(Tuition)	-0.006 (0.048)	-0.070 (0.051)	-0.023 (0.050)	✓	-0.022 (0.049)	-0.240*** (0.062)	-0.124* (0.064)	✓
Log(Students), Change	0.017* (0.010)	0.001 (0.009)	-0.006 (0.009)	✓	0.021* (0.011)	0.001 (0.011)	-0.009 (0.007)	✓
For Profit	0.052 (0.035)	-0.003 (0.040)	0.025 (0.028)	✓	0.006 (0.028)	0.031 (0.057)	0.100** (0.047)	✓
Share Black	0.157 (0.233)	-0.028 (0.159)	0.241 (0.157)	✓	0.594 (0.388)	0.044 (0.521)	0.423 (0.357)	✓
Share Hisp.	-0.172 (0.141)	0.548*** (0.144)	0.763*** (0.117)	✓	-0.287 (0.223)	0.358 (0.362)	0.799*** (0.279)	✓
Share Bach.	-0.116 (0.133)	0.074 (0.100)	0.273*** (0.080)	✓	0.028 (0.135)	0.664*** (0.163)	0.792*** (0.145)	✓
Risk Score					-0.041 (0.029)	-0.024 (0.042)	-0.094** (0.048)	✓
Age					0.002 (0.037)	-0.013 (0.041)	0.006 (0.038)	✓
Debt to Income					-0.052 (0.032)	0.238 (0.191)	-0.082* (0.045)	✓
Card Util.					0.026* (0.016)	-0.039 (0.030)	-0.017 (0.024)	✓
Mortgage Delinq.					0.013 (0.015)	-0.009 (0.019)	-0.022 (0.019)	✓
R-squared	0.7	0.3	0.5		0.8	0.4	0.6	

Notes: MSA-clustered standard errors in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

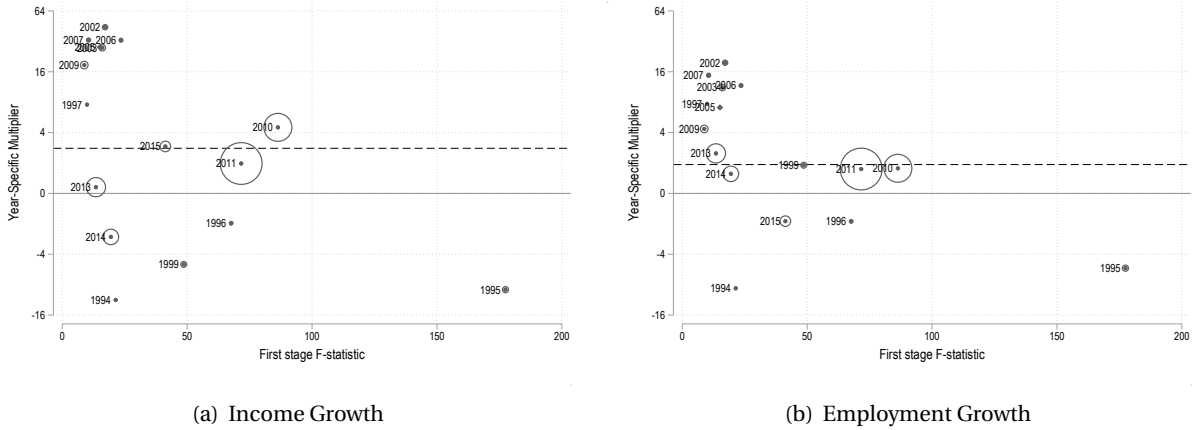
3.2.2. Rotemberg Weights

In a second validation exercise, we analyze whether particular years drive our identification using so-called Rotemberg weights. Goldsmith-Pinkham et al. (2020) show that shift-share IV estimates can be written as a weighted sum of GMM estimations using the recipient shares interacted with time fixed-effects as separate instruments. Rotemberg weights are the weights of observations in this decomposition. The weights depend on the covariance between the specific instrument's fitted value of the endogenous variable and the endogenous variable itself.²¹ High-weight years are therefore the years to which the SSIV estimate is most sensitive. If these years also have strong first stages, their just-identified IV estimates should ideally be close to the overall multiplier estimate. Otherwise, the overall estimate may be driven by a few outlier years, raising concerns that the result reflects year-specific endogeneity.

We plot the Rotemberg weights in Figure 2. Because we are interested in all years of the data, we present the estimates for the full specification of the all-year sample in Table 2 (column 3). The figure plots the just-identified yearly estimates of the multiplier against the first-stage F-statistic for that year. Each point's size is proportional to that year's weight, while the horizontal line is the overall multiplier from Table 2.

²¹In the simplest example where the yearly instruments are all orthogonal, the weights are simply the ratio between the just-identified first-stage R^2 and the full SSIV first-stage R^2 . In our case, the multiplier estimates can be interpreted as a weighted sum of just-identified estimates calculated using the MSA-level Pell recipients population share for each year ($s_{m,t-2}$) separately.

Figure 2. Rotemberg Weights and Multiplier Estimates from Single-Year Instruments



Notes: The figure plots each year's multiplier estimate against that year's first-stage F-statistic. The size of the circles and diamonds is scaled by the magnitude of the respective Rotemberg weight. Circles denote positive weights while diamonds denote negative weights. Dashed lines are multipliers from Table 2, column 3. Excludes instruments with F-statistics below 5.

The figure shows that the years with the largest Rotemberg weights are 2010 and 2011, because the Pell grant program expanded rapidly in those years (see Appendix Figure A4). These year-specific multipliers are close to the baseline estimate and have a high F-statistic. This implies that our estimate of the multiplier is not noticeably biased due to endogeneity of the instruments in a particular year. While we do observe some implausible estimates, the outliers carry a low Rotemberg weight and a low F-statistic, so that those years do not significantly affect the overall estimate. Moreover, while there are points with negative weights, these account for little of the overall weight. Taken together, the overall estimates are clearly driven by years with strong first stages and multipliers close to the baseline, as required.

3.2.3. Further Validity Tests

Online Appendix B presents results for four additional validity tests, which we briefly summarize below.

Geographical Balance Appendix Figure A1 illustrates the geographical distribution of Pell grant recipients. It is well established that poorer areas grow faster than richer areas in the U.S. (see, e.g., Barro et al., 1991). If Pell grant recipient shares are higher in areas with low or high growth, that would be an example of an observable variable that correlates with both our shares and the dependent variable, violating Goldsmith-Pinkham et al. (2020)'s balance test. We find that the distribution does not exhibit geographical clustering. Most states contain both MSAs with above- and below-average Pell grant recipient shares, in support of the requirement that there is a balanced instrument distribution across regions.

Bias from Persistent Effects We also assess whether we are subject to biases that arise as a result of endogenous dynamic responses to the shift-share instruments (Appendix Table A4). Jaeger et al. (2018) explain that Table 2's estimates will be biased if two conditions are met. First, the instrument is serially correlated. Second, the multiplier effects induced by the Pell disbursements affect future local GDP, as the economy takes some time to adjust to changes in the Pell grants. If both of these conditions are met, our multipliers may be confounded by the long-term effects of older changes to Pell grants. Jaeger et al.

(2018)’s solution entails controlling for one-year lagged Pell grants and instrumenting them with one-year lagged SSIV. We find no significant differences from our baseline in Table 2—if anything, estimates are higher, confirming that there is no significant dynamic bias in our coefficients.

Using Shares as Instrumental Variables Next, we estimate multipliers using the interaction of time fixed effects and our shares variable—the lagged share of Pell grant recipients in the population—as the instrument for Pell grants in the multiplier estimation (Appendix Table A5). Goldsmith-Pinkham et al. (2020) propose this test, to verify that our multiplier estimates are driven by variation in the shares variable and not primarily by the shocks to the national Pell grants program. Practically, this approach estimates the multiplier from an *unweighted* average of the points in Figure 2, instead of weighting by growth in national Pell grants, and should yield similar results to the baseline. We find that all multiplier estimates are qualitatively similar to Table 2’s using this “shares only” approach for the post-1999 sample.²² When including the earlier years the multipliers are mostly lower. This may be because we use a proxy variable for the Pell recipients in the first part of the full sample, constructed using MSA-year variation. As such, the fixed effects of the “shares only” multiplier estimation are likely to absorb the variation in that proxy.

Mean Reversion Finally, we perform extensive checks to ensure that mean reversion is not driving the multiplier in addition to lagged dependent variable specifications in the main table. Because Pell grant eligibility is tied to the lower end of the income distribution, one might argue that a local downturn both expands eligibility and creates the conditions for a subsequent rebound.

First, we re-estimate the preferred specifications in Table 2 using alternative controls for the initial state of the local economy, both in levels and in differences: the output gap, the unemployment rate, the poverty rate, and the unemployment gap (Appendix Figure A2, see figure notes for variable definitions). These controls have no clear effect on the estimated multipliers in either direction.

Second, we assess the effect using alternative lags of the Pell grant recipient shares when constructing our shift-share instruments. The baseline uses the fraction of the local population that receives a Pell grant at time $t - 2$ and studies Pell’s effect between $t - 2$ and t . Appendix Table A6 shows that multipliers are somewhat higher when using the shares of the local population from the last time the local economic area experienced *positive* personal income growth. These shares are at least from $t - 2$, but they tend to have longer lags especially in the aftermath of recessions. In addition, we have done other similar exercises, including using shares from $t - 3$ and other permutations of the exercise that we did in Table A6. Across these exercises, we consistently find similar or greater multipliers than the baseline.

Third, we have verified that Pell grants’ multiplier remains large when excluding the Great Recession from the sample. Appendix Figure A3 shows that doing so raises the multiplier for every specification, though standard errors are much larger—which is consistent with the Rotemberg weights in Figure 2.

²²Note that in the presence of time state dependence, e.g., when multipliers are different in recessions and expansions, the weighted and unweighted estimates do not need to match. We discuss state-dependence further in Section 5.

4. Mechanisms

What drives the multiplier effect of Pell grants? A leading explanation for large fiscal multipliers is the Keynesian expenditure mechanism: government spending raises private income, and households spend part of that additional income, generating further rounds of demand (Kahn, 1931; Auclert et al., 2024). At the same time, Pell grants may reduce recipients' labor supply through an income effect, attenuating the expansion in employment generated by higher demand. More generally, the extent to which Pell-grant-induced demand translates into higher local output and employment depends on the availability of underutilized resources and the elasticity of local factor supply (e.g., Auerbach et al., 2020).

We perform a series of decompositions to distinguish three parts of the mechanism: whether Pell grants create a local demand impulse, how local labor markets accommodate that impulse, and which spending channels generate it. First, we ask whether Pell grants raise local demand. We show that the employment response is concentrated almost entirely in non-tradable sectors, where activity is closely tied to spending by local consumers. Second, we ask how local labor markets accommodate this increase in demand. Employment rises among workers aged 22 and over but falls modestly among younger workers, consistent with a negative income effect on students' labor supply. The aggregate employment gain reflects both lower unemployment and higher labor-force participation. Finally, we ask what generates the initial demand impulse, using an accounting decomposition that places bounds on the contributions of student spending, spending by educational institutions, and subsequent rounds of local spending.

4.1. Employment in Tradable and Non-Tradable Sectors

We start by splitting the overall employment rate response of Pell grants into tradable and non-tradable sectors. If the positive effects of Pell grants operate mainly by increasing local spending, we expect the employment response to be concentrated in non-tradable sectors, where production is tied more closely to local demand (Moretti, 2010). We use MSA-level employment by industry from the Current Employment Statistics and construct sector-specific employment changes over the same two-year horizon as in the baseline specification. For each sector $s \in \mathcal{S}$, we estimate

$$\frac{\Delta l_{m,t;t-2}^s}{l_{m,t-2}} = \beta^s \left(\frac{\Delta e_{m,t;t-2}}{y_{m,t-2}} \right) + \phi_m + \psi_t + \gamma' x_{m,t-2} + \mu_{m,t}^s, \quad (4)$$

where $\Delta l_{m,t;t-2}^s$ denotes the change in employment in sector s in metropolitan area m over the prior two years, while $l_{m,t-2}$ is the area's total initial employment across all sectors. Because each sector-specific employment change is divided by the same overall initial level of employment, the sector-specific employment multipliers β^s sum to the full employment multiplier of Pell grants. However, because the CES sector data are not available for every MSA-year in our main sample, the overall employment multipliers in the sectoral tables differ somewhat from the baseline employment multiplier.

We classify employment into tradable and non-tradable sectors using a broad industry-based classification. Following Acemoglu et al. (2016), we define tradable employment as employment in manufacturing and mining/logging, and classify all remaining CES nonfarm employment as non-tradable. The

Table 4: Decomposition of the Pell Employment Multiplier into Sector-Level Employment

	Full Sample		Post 1999	
	Effect on Employment	Percentage of Total	Effect on Employment	Percentage of Total
Tradables	0.110 (0.459)	6.3%	0.358 (0.442)	16.4%
Non-Tradables	1.633 (1.142)	93.7%	1.820 (1.136)	83.6%
Total	1.743 (1.257)	100%	2.178* (1.277)	100%

Notes: Full sample: N=7,134; post-1999 sample: N=3,941. Samples change due to CES data availability. Each row presents the subgroup-specific employment multiplier estimate. 2SLS estimates follow (4). Controls follow specifications (3) and (9) in Table 2. Clustered standard errors in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

non-tradable sector therefore includes construction, wholesale and retail trade, education and health, government, and other service-sector employment. This classification is deliberately broad: it captures whether the employment response occurs in sectors whose activity is more likely to be tied to local demand rather than production sold outside the metropolitan area.

Table 4 presents the decomposition for the preferred specification, which shows that the employment response is almost entirely concentrated in the non-tradable sector.²³ In the full sample, non-tradable employment accounts for 1.63 percentage points of the 1.74 employment multiplier, or 94% of the total effect. Tradable employment contributes only 0.11 percentage points. The same pattern holds in the post-1999 sample, where the non-tradable response is even larger in levels, at 1.82 percentage points, accounting for 84%. Given that non-tradable goods and services are produced locally, the sectoral decomposition points to an increase in local demand as the driver of the Pell multiplier.

We also estimate the decomposition for more detailed CES sectors. These come with relatively large standard errors, so we do not report them in a separate table. Still, the pattern is informative. The positive employment response is mainly accounted for by construction and wholesale and retail trade, two sectors that should respond strongly to local spending. In contrast, employment in education and health responds little. This suggests that the effect is not primarily driven by colleges or the education sector hiring more workers when Pell grants increase. Rather, the detailed sectoral results reinforce the interpretation that Pell grants raise employment by increasing local demand.

4.2. Margins of Labor Market Adjustment

We next ask how local labor markets accommodate the increase in demand generated by Pell grants. In particular, we distinguish between two margins through which employment can expand: a reduction in unemployment and an increase in labor-force participation. Using Local Area Unemployment Statistics (LAUS) from BLS and MSA-level population statistics, we construct the change in the number of unemployed workers and the change in the number of people outside the labor force. We divide each change by initial employment, matching the normalization used in equation (4). Because declines in unemployment and nonparticipation both raise employment, we report the negatives of the estimated coefficients

²³These are columns (3) and (9) of Table 2. Results for other specifications are similar, and are found in Appendix Figure A5.

Table 5: Decomposition of the Pell Employment Multiplier into Unemployment and Participation

	Full Sample		Post 1999	
	Effect on Employment	Percentage of Total	Effect on Employment	Percentage of Total
Unemployment	1.142 (0.720)	59.1%	0.829 (0.766)	31.4%
Not In Labor Force	0.790 (1.151)	40.9%	1.807 (1.181)	68.6%
Total	1.932* (1.148)	100%	2.636** (1.267)	100%

Notes: Full sample: N=8,388; post-1999 sample: N=4,447. Samples change due to LAUS data availability. Each row presents the subgroup-specific employment multiplier estimate. 2SLS estimates follow (4). Controls follow specifications (3) and (9) in Table 2. Clustered standard errors in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

as positive contributions to the employment multiplier. Since these variables are available for most MSA-years, the sum of components is close to the employment multipliers reported in Table 2.

Results for the preferred specifications are presented in Table 5; results for the remaining specifications are qualitatively similar and are reported in Figure A7 of Online Appendix C. In the full sample, lower unemployment contributes 1.14 percentage points to the employment response, or 59% of the total employment multiplier. The remaining 0.79 percentage points, or 41%, comes from a reduction in the number of people outside the labor force. In the post-1999 sample, the relative importance of the two margins reverses: lower unemployment contributes 0.83 percentage points, or 31% of the total effect, while higher participation contributes 1.81 percentage points, or 69%.

Although both components are estimated with sizable standard errors, the decomposition indicates that the employment response is not concentrated in a single labor-market margin. Pell grants raise local employment through both lower unemployment and higher labor-force participation. The larger participation contribution in the post-1999 sample may partly reflect the greater weight placed on the Great Recession, when underutilized labor was particularly abundant.

4.3. Employment Response by Age

Having shown that Pell grants raise local demand and that local labor markets accommodate this demand through both lower unemployment and higher participation, we next examine a potentially offsetting force: a reduction in recipients' labor supply. Because Pell grants raise students' non-labor income, they may reduce students' need to work while enrolled, consistent with the negative labor-supply response documented by Kofoed (2022). We therefore decompose the employment multiplier by age, asking whether employment falls among workers in the typical college-age range.

We implement the age decomposition using the same specification as in equation (4). For each age group $a \in \mathcal{A}$, we replace the sector change in employment with the change in employment among workers in age group a , again normalized by the area's total initial employment. This common normalization ensures that the age-specific employment multipliers sum to the total employment multiplier in the available sample. We obtain employment by age from the Quarterly Workforce Indicators (QWI).²⁴

²⁴Note that QWI tracks jobs and not employment. We use the share of stable jobs in a given age group for a specific MSA multiplied by the LAUS employment estimate for that MSA as a proxy for the employment by age group in a given MSA.

Table 6: Decomposition of the Pell Employment Multiplier into Age-Specific Employment

	Full Sample		Post 1999	
	Effect on Employment	Percentage of Total	Effect on Employment	Percentage of Total
<i>Age Group</i>				
Age 14-18	-0.013 (0.128)	-0.5%	0.127 (0.117)	4.8%
Age 19-21	-0.083 (0.166)	-3.2%	-0.110 (0.151)	-4.2%
Age 22-24	0.384 (0.247)	14.6%	0.336* (0.199)	12.8%
Age 25-34	0.659 (0.415)	25.0%	0.575 (0.370)	21.9%
Age 35-44	0.630** (0.273)	23.9%	0.566** (0.268)	21.5%
Age 45-54	0.499 (0.362)	19.0%	0.606* (0.367)	23.1%
Age 55+	0.557** (0.240)	21.2%	0.528** (0.240)	20.1%
Total	2.632** (1.258)	100%	2.628** (1.270)	100%

Notes: Full sample: N=5,977; post-1999 sample: N=4,352. Samples change due to QWI data availability. Each row presents the age-specific employment multiplier estimate β^a . 2SLS estimates follow (4). Controls follow specifications (3) and (9) from Table 2. Clustered standard errors in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 6 presents the effect of Pell grants on employment by age. We use narrow age bins for younger workers and broader bins for workers aged 25 and over. The left-hand panel presents results for the preferred specification in the full sample, while the right-hand panel presents results for the preferred specification in the post-1999 sample. The pattern is similar across the remaining specifications (see Figure A6 in Online Appendix C). Because QWI coverage is limited for some MSAs early in the sample, the implied total employment multipliers differ somewhat from those in Table 2, particularly in the longer full sample. We therefore focus on the percentage contribution of each age group to total employment.

Across both panels, the point estimates are positive for every age group aged 22 and over. By contrast, the employment response among workers aged 19–21—the age group with the most Pell-grant-eligible students—is modestly negative. Although this estimate is imprecise, the pattern is consistent with a negative income effect on students’ labor supply documented by Kofoed (2022). The age decomposition therefore suggests that the aggregate employment multiplier is driven by employment rises among older workers in the local economy that offset a potential reduction in labor supply among likely students.

4.4. Sources and Propagation of the Income Multiplier

The preceding employment decompositions show that Pell grants raise local demand and employment, but not where the initial spending comes from or why the income response exceeds the direct transfer. This subsection presents an illustrative decomposition of the income multiplier into its potential drivers: the mechanical contribution of Pell grants to measured personal income, the first-round income generated by spending out of the additional resources of students and educational institutions, and a residual capturing further rounds of local propagation.

The BEA measures personal income in metropolitan area m as $y_{m,t} = e_{m,t} + g_{m,t} + y_{m,t}^F$: the sum of Pell grant transfers attributed to the MSA, other government transfers, and local factor income. Because the BEA attributes Pell grants to the location of the recipient's school (BEA, 2025), Pell grants raise measured personal income one-for-one through $e_{m,t}$. Our preferred specifications hold the main non-Pell transfers constant through extensive controls, hence any multiplier in excess of one must operate through local factor income $y_{m,t}^F$. Thus, we decompose the multiplier into the following components:

$$\beta = \frac{\partial(\Delta y_{m,t;t-2}/y_{m,t-2})}{\partial(\Delta e_{m,t;t-2}/y_{m,t-2})} = \underbrace{1}_{\text{Mechanical effect}} + \underbrace{\lambda_s \frac{\partial \Delta y_{m,t;t-2}^s}{\partial \Delta e_{m,t;t-2}}}_{\text{First-round: students}} + \underbrace{\lambda_i \frac{\partial \Delta x_{m,t;t-2}^i}{\partial \Delta e_{m,t;t-2}}}_{\text{First-round: institutions}} + \underbrace{\mu}_{\text{Further propagation}}; \quad (5)$$

where $y_{m,t}^s$ denotes the resources available to students and $x_{m,t}^i$ expenditures by educational institutions. The first term is the mechanical effect of Pell grants on personal income. The propensities λ_s and λ_i in the second and third term give the first-round increase in local factor income per dollar of student resources and institutional spending, respectively, and the residual μ captures all subsequent rounds of local spending, net of any attenuation from local supply constraints.²⁵

To quantify this decomposition, we estimate the responses of student resources and institutional expenditures to Pell grants using the same shift-share design as in our baseline analysis. We use evidence from prior work to discipline the plausible bounds of the local spending responses λ_s and λ_i , which we cannot estimate directly in our setting. Given these estimates and parameter values, we recover μ as the residual between the total income multiplier and the mechanical and first-round effects.

Student resources. We begin with the student channel. The effect of Pell grants on students' disposable resources follows from the financing identity of the students' budget. A student finances the cost of college and consumption out of grants, loans, and own resources, net of tuition payments. Because grants, loans, and tuition are jointly determined in the financial-aid package—students apply for all Title IV aid through a single FAFSA application, and colleges assemble aid offers to cover the gap between the cost of attendance and the expected family contribution—a change in Pell grants mechanically operates on all three margins of this identity rather than on grant income alone. Prior work documents both the loan and tuition margins. First, by making college attendance feasible, Pell grants unlock access to student loans (Radwin et al., 2018). Second, colleges may capture part of the additional aid through higher net tuition, particularly in the for-profit sector.²⁶ Letting $\ell_{m,t}$ denote student-loan disbursements and $\tau_{m,t}$ net tuition payments, the response of students' disposable resources to Pell grants is:

$$\frac{\partial \Delta y_{m,t;t-2}^s}{\partial \Delta e_{m,t;t-2}} = 1 + \frac{\partial \Delta \ell_{m,t;t-2}}{\partial \Delta e_{m,t;t-2}} - \frac{\partial \Delta \tau_{m,t;t-2}}{\partial \Delta e_{m,t;t-2}}. \quad (6)$$

²⁵Because the changes in income and Pell grants on the right-hand side are normalized by the same level of initial income, this normalization cancels in the multiplier. Each component therefore gives dollars of local income per additional Pell dollar.

²⁶Previous work finds that colleges, particularly for-profit institutions, raise tuition in response to expansions in federal financial aid; see Cellini and Goldin (2014) and Lucca et al. (2019).

We estimate both derivatives with the same shift-share design as the baseline. For each MSA-year, we sum student-loan disbursements and net tuition payments across all schools, and replace personal income in equation (2) with that variable. We find a loan response of approximately $\partial\Delta\ell/\partial\Delta e = 1.98$ for the preferred specification, so that each dollar of Pell grants is associated with roughly two additional dollars of student-loan disbursements. The net tuition response is $\partial\Delta\tau/\partial\Delta e = 2.28$. Substituting into equation (6), a dollar of Pell grants raises students' disposable resources by approximately \$0.70.

In Appendix D, we provide evidence on the drivers of the rise in student loans and institutional spending. There, we show that Pell grants expand the number of students that apply and enroll in colleges (Appendix Table A10). This evidence reconciles our findings with the conclusion in Marx and Turner (2018) that Pell grants *crowd out* borrowing at the individual level: Grants also enable students who otherwise would not have attended college to enroll, and these marginal entrants take out new loans (Dynarski, 2003). Using the same shift-share design applied at the level of individual institutions, the appendix presents school-level regressions showing that an increase in Pell grants equal to 1% of a school's tuition receipts raises applications by 0.7% and enrollment by 1.3% at non-profit colleges. At the level of a metropolitan area, this extensive-margin response appears to exceed the individual-level crowding out.²⁷

Institutional expenditures. We next turn to the institutional component of equation (5). Pell grants may raise institutional spending both because the tuition payments captured by schools increase the resources available to them and because grants induce enrollment that would not otherwise occur. Consistent with this channel, Dinerstein et al. (2014) find that public universities increased educational expenditures following an expansion of Pell grants. For each MSA-year, we sum expenditures on instruction, student services, research, and public service across all schools in the MSA, and estimate the response of this total to Pell grants using the same specification as above. The resulting coefficient, $\partial\Delta x^i/\partial\Delta e = 1.25$, measures the dollars of additional institutional spending per dollar of Pell grants. Note that equation (5) does not double count the tuition captured by schools: the tuition response is subtracted from student resources in equation (6) and re-enters only insofar as institutions spend it.

Calibration of propensities λ_s and λ_i . We present the decomposition for three values of λ_s , which measures the first-round increase in local factor income per dollar of student resources. In the first round, this is simply the fraction of the marginal dollar that students spend on locally produced goods and services: the marginal propensity to consume (MPC), scaled by the local content of that consumption. Because students spend predominantly on non-tradables produced within the metropolitan area—housing, groceries, transportation, and local services—the local content of their marginal spending is likely close to one, so λ_s can be disciplined directly by estimates of the MPC. We therefore consider three values that span the range in the literature. As a lower bound, we take $\lambda_s = 0.30$, corresponding to the quarterly spending response to the 2008 economic stimulus payments estimated for the general population by Parker et al. (2013). Our central value, $\lambda_s = 0.48$, is the average self-reported MPC in Jappelli and Pistaferri (2014), who further show that the MPC rises substantially for households with low cash on hand—

²⁷Panel (c) of Appendix Table A10 shows that Pell grants furthermore raise the share of minority students in enrollment, consistent with grants drawing in students from underrepresented, lower-income backgrounds.

precisely the situation of most Pell recipients. As an upper bound, we take $\lambda_s = 0.71$, in line with the MPC of hand-to-mouth households in [Carroll et al. \(2017\)](#). This upper bound is plausible in our setting because a large share of the marginal student resources consists of loans taken out expressly to finance current college-related expenses, so the propensity to spend out of these resources may approach one.

The propensity λ_i gives the first-round increase in local factor income per dollar of institutional expenditure. Because our expenditure measure comprises instruction, student services, research, and public service, the marginal dollar goes largely to compensation: salaries and benefits account for roughly 60–75% of core educational spending at U.S. institutions ([Desrochers and Hurlburt, 2016](#); [Snyder et al., 2019](#)), and college employees overwhelmingly reside in the metropolitan area where the institution is located. The remainder is split between locally procured services and supplies purchased from outside the MSA. We therefore set $\lambda_i = 0.7$, allowing for leakage of non-labor spending.

Results. Table 7 presents the results. We only perform the decomposition for the post-1999 sample because few MSAs have institutional expenditure data before 2000. The top panel shows that each dollar of Pell grants is associated with approximately \$1.98 of additional student-loan disbursements, \$2.28 of additional net tuition payments, and \$1.25 of additional institutional expenditures. The bottom panel translates these estimates into contributions to the multiplier. The mechanical effect of Pell grants on measured personal income accounts for 36.8% of the multiplier; close to two-thirds reflects a response of local factor income. First-round spending by institutions contributes a further 32.2%, while the student channel contributes 7.6% to 18.1%, depending on the λ_s calibration. Because the direct and institutional contributions come from separate elasticities, a higher student propensity to consume raises students' contribution at the expense of the residual, which falls from 23.4% to 12.9% across the calibrations.

We can use the interpretation of the residual to calculate the implied marginal propensity to consume. Absent local supply constraints, the residual captures the effect of further rounds of spending, which compound at the average local marginal propensity to consume.²⁸ The residual contributions in Table 7 then imply average local MPCs of roughly 0.26–0.39, well within the range of average MPCs estimated in the literature ([Johnson et al., 2006](#); [Parker et al., 2013](#); [Jappelli and Pistaferri, 2014](#)).

4.5. Taking Stock

The decompositions in this section paint a consistent picture of why Pell grants raise local economic activity by more than the size of the initial transfer. Pell grants raise local demand: the employment response is concentrated almost entirely in non-tradable sectors, while employment in the education sector itself responds little. Local labor markets accommodate this demand through both lower unemployment and higher labor-force participation. The gains accrue to workers aged 22 and over, whereas employment among those aged 19–21 falls modestly, consistent with a negative income effect on students' labor supply. The aggregate multiplier therefore does not reflect students working more when grants increase, but the local economy responding to their spending.

²⁸With a constant marginal propensity to consume, each dollar of first-round factor income generates $MPC + MPC^2 + \dots = MPC/(1 - MPC)$ dollars of further local income.

Table 7: Decomposition of the Pell Grant Personal Income Multiplier

	Post 1999		
Pell grant income multiplier	2.715*		
	(1.535)		
Effect of Pell grants on student loans	1.975***		
	(0.444)		
Effect of Pell grants on tuition	2.284**		
	(0.969)		
Effect of Pell grants on institution expenditures	1.248**		
	(0.537)		
<i>Implied contributions to Pell grant multiplier</i>	$\lambda_s=0.30$	$\lambda_s=0.48$	$\lambda_s=0.71$
Direct effect	36.8%	36.8%	36.8%
Institution spending contribution	32.2%	32.2%	32.2%
Student spending contribution (by λ_s)	7.6%	12.2%	18.1%
Residual (propagation; supply constraints) (by λ_s)	23.4%	18.8%	12.9%
	100%	100%	100%

Notes: Post-1999 sample: N=4,357. Compared to Table 2, samples change due to data availability of additional variables. The top panel contains separate 2SLS estimates with controls following specification (9) in Table 2. MSA-clustered standard errors in parentheses. Contributions follow eq. 5 and assume $\lambda_i = 0.7$ and divide each term in the equation by the total multiplier estimates in the top panel. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

The income decomposition identifies the sources of that spending. Roughly a third of the multiplier reflects the mechanical contribution of Pell grants to measured personal income; the remainder operates through local factor income. Each dollar of grants raises both student-loan disbursements and institutional expenditures, so that first-round spending by students and institutions accounts for a further 40 to 50% of the multiplier. Finally, the residual is consistent with estimates of the local marginal propensity to consume in the literature. Two features thus distinguish Pell grants from other forms of government spending: grants unlock complementary financing through student loans and they channel resources through educational institutions. Because the institutional spending channel appears so prominent, these results naturally raise the question of whether the multiplier depends on the type of institution that recipients attend—and, given the role of slack in local propagation, on the state of the local economy. Section 5 turns to these questions.

5. Heterogeneity

We next investigate the conditions under which the local multiplier effects of Pell grants are large, focusing on two sources of heterogeneity. First, we examine whether the multiplier varies across types of educational institutions, since the results in the previous section show that educational spending is a key driver of the overall effect of Pell grants. Second, we examine whether the multiplier differs between expansions and recessions, since the extent to which economic activity can increase depends on the availability of slack resources over the business cycle. In other words, we answer where and when a taxpayer dollar of Pell grants generates the largest increase in local activity.

5.1. Estimation Equation

To estimate heterogeneity in the effect of Pell grants on local economic activity, we estimate a “smooth transition model.” The smooth transition model builds on our main estimation equation (2), but interacts the growth rate of Pell disbursements with variables that capture heterogeneous characteristics of the local economy, such as the prevalence of different types of educational institutions or indicators of whether the local economy is in recession. The estimation equation reads:

$$\frac{\Delta y_{m,t;t-2}}{y_{m,t-2}} = F(z_{m,t-2}) \left[\bar{\alpha} + \bar{\beta} \frac{\Delta e_{m,t;t-2}}{y_{m,t-2}} \right] + [1 - F(z_{m,t-2})] \left[\underline{\alpha} + \underline{\beta} \frac{\Delta e_{m,t;t-2}}{y_{m,t-2}} \right] + \phi_m + \psi_t + \gamma' x_{m,t-2} + \mu_{m,t}, \quad (7)$$

where the function $F : \mathbb{R} \rightarrow [0, 1]$ transforms some initial state of the local economy, $z_{m,t-2}$, such that $\bar{\beta}$ and $\underline{\beta}$ capture the multiplier when the initial state of the economy is at the minimum and maximum value of $z_{m,t-2}$, respectively. We estimate equation (7) using two-stage least squares, instrumenting Pell grant disbursements at the MSA level with the same instruments as in our main regressions, multiplied by $1 - F(z_{m,t-2})$ for the low-state term and by $F(z_{m,t-2})$ for the high-state term. To elucidate what drives variation in the Pell multipliers, we further estimate equation (7) with student loans, tuition income, and institutional spending as the dependent variable, as we did for the overall Pell multiplier in Table 7.

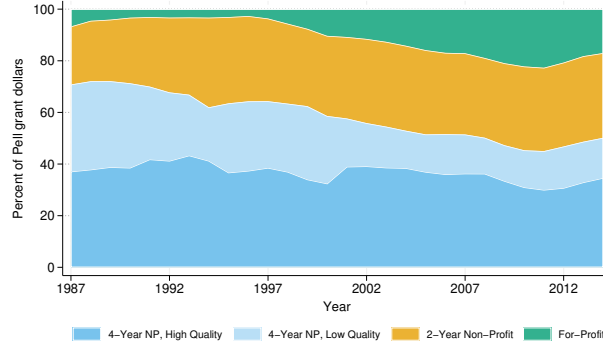
5.2. Multiplier Heterogeneity by Institution Type

We begin by asking whether the Pell multiplier depends on the characteristics of an MSA’s educational institutions. We consider four dimensions of heterogeneity that are likely to shape how institutions respond to grants in terms of their tuition and expenditures: whether a school is non-profit or for-profit, whether it primarily offers two-year or four-year degrees, whether it ranks in the top half of the academic quality distribution, and the extent to which it enrolls undergraduate or post-baccalaureate students.²⁹ For-profit colleges may have different multipliers because, if they have market power, such colleges may be able to charge higher tuition fees in response to higher generosity of Pell grants. Pell grants can therefore operate as an implicit subsidy. As public companies own a large fraction of for-profit colleges, not all of these tuition revenues will be spent within the college’s MSA, reducing local economic effects.³⁰ The other variables matter because they shape both how much of a grant reaches students’ budgets and what schools do with the associated tuition revenue. Two-year colleges are typically community colleges that offer post-secondary education to local students. Four-year colleges are often more broadly engaged in academic activities, including research. Highly ranked institutions are therefore typically larger local employers with a wider range of expenditures—on instruction, student services, and research—that accrue to the local economy. Students at these institutions also remain enrolled longer, which may prolong the period over which grants relax budget constraints and unlock complementary financing such as student

²⁹We define an index of college quality as the normalized college quality index in Carruthers and Welch (2019), whose index is calculated as the mean of five quality measures that are transformed to have mean equal to zero and a standard deviation equal to one: 25th and 75th percentile ACT intake, instructional expenditures per FTE, faculty-student ratio, and graduation rate.

³⁰Examples of publicly listed companies that own for-profit colleges are Grand Canyon University (LOPE), Adtalem (ATGE, previously DeVry), American Public University System (APEI), and Bridgepoint Education Inc. (BPI).

Figure 3. Percentage of Pell Grants Awarded to Different Types of Schools



Notes: Figure plots the fraction of national-level Pell grants that is awarded to students who are enrolled in for-profit institutions, 2-year non-profit institutions, 4-year non-profit institutions, split between high and low quality. High quality schools are defined as schools that score above median across Carruthers and Welch (2019)'s indices.

loans. Finally, post-baccalaureate students qualify for Pell grants only in select programs and typically have access to alternative funding, so grants awarded to them may do less to relax budget constraints.

Before turning to the multiplier estimates, Figure 3 plots the distribution of Pell grant awards across these types of institutions. We distinguish between awards to for-profit colleges, non-profit two-year colleges, non-profit four-year colleges of above-median quality, and non-profit four-year colleges of below-median quality.³¹ Three patterns stand out. First, the largest share of Pell grants is awarded to students in four-year programs at institutions that are in the top half of the institution quality distribution, and this share has been constant at around 40% over the sample. Second, the share of Pell grants awards to for-profit colleges increased from a negligible percentage to 15–20% between 2000 and 2010, at the expense of non-profit colleges in the bottom half of the quality distribution.³² Third, the share of Pell grants going to two-year colleges has been relatively constant at around 30% of total disbursements.

Table 8 assesses how these institutional differences affect the Pell multiplier. The first panel presents the baseline multiplier as well as the effect of Pell grants on student loans, tuition, and institutional expenditures, taken from Table 7. In subsequent panels we estimate the state-dependent multipliers along eq. (7). Each panel simply defines $F(z_{m,t-2})$ as the fraction of students enrolled in that panel's particular type of educational institution. The estimates of $\underline{\beta}$ and $\bar{\beta}$ are therefore the multipliers in the hypothetical scenario where either *none* or *all* students in an MSA are enrolled in that panel's type of institution.

Panel (a) shows that the Pell multiplier is lower when a large fraction of Pell grant recipients attend for-profit institutions. Although standard errors are large, the point estimates show that the multiplier is 1.95 in cities where all recipients attend for-profit schools, and 3.08 when no recipients attend for-profit schools. The institutional response to an expansion in Pell grants is responsible for the wedge: for-profit colleges raise their tuition fee revenues around four times as much as non-profit institutions, and therefore eliminate the positive effect of Pell grants on students' disposable income. This is consistent

³¹We do not estimate separate multipliers for two-year and four-year for-profit colleges, or for for-profit colleges of above- and below-median quality, because these subgroups receive a minor fraction of overall Pell grants.

³²The reduction after 2013 is the result of "Gainful Employment" regulation, which restricts federal student aid at several for-profit institutions (see, for example, Cellini et al., 2016).

with the hypothesis that Pell grants act as an implicit subsidy for such schools. The additional tuition income is not spent since institutional spending rises by a similar amount at both for-profit and non-profit schools.³³

Turning to Panel (b), we find that the Pell multiplier is lower when recipients are enrolled in two-year degrees: the multiplier for an all-two-year city is (insignificantly) negative, while the multiplier at an all 4-year city is 3.75. The supplementary regressions show that this appears to be driven by the fact that two-year institutions do not increase institutional spending when Pell grants expand. Tuition increases are also smaller at such colleges, but because the increase in student loan disbursement falls by a similar amount, the impact of Pell grants on disposable income is similar at two-year versus four-year schools.

In Panels (c), (d), and (e) we assess whether school quality affects the Pell multiplier. Panel (c) shows that in a city in which all Pell grant recipients attend schools in the bottom 50% of the quality distribution the multiplier is 2.01, versus 3.22 if all recipients attend schools in the top 50%. The difference is driven by two factors: students at good colleges are willing to take out more student loans, and good colleges raise their spending almost proportionally to their tuition income. Weaker colleges, by contrast, only raise their spending by a third of the increase in tuition income. Panels (d) and (e) show that these patterns remain when comparing 4-year high-quality colleges, at which the Pell multiplier is 2.96, to any other type of college, or when comparing 4-year low-quality colleges.

Finally, Panel (f) compares the Pell multiplier at colleges whose student body consists predominantly of undergraduate versus post-baccalaureate students. As Pell grants are nearly exclusively awarded to undergraduate students, we find that the effect of grants in areas where students are predominantly enrolled in post-baccalaureate programs is estimated with very large error. Hence, our results should be interpreted as being about undergraduate students.

Overall, the results in Table 8 show that a dollar of Pell grants is most effective at raising local economic activity when it is awarded to students at high-quality, non-profit, four-year colleges. Although these colleges raise tuition fees when Pell grants become more generous, they also increase institutional spending almost proportionally, and the accompanying rise in student loan disbursements ensures that students' disposable income increases. Both channels of the multiplier—institutional expenditures and student spending—are therefore active at these institutions, in contrast to for-profit colleges, where tuition increases absorb the grant, and two-year colleges, where institutional spending does not respond.

The school-level regressions in Appendix D corroborate these patterns on the student side. At non-profit institutions, Pell grants raise applications, enrollment, and the share of minority students in enrollment (Table A10), with the enrollment response concentrated at four-year and above-median-quality schools—precisely the institutions with the highest multipliers. At for-profit institutions, applications do not respond and enrollment declines, consistent with tuition increases absorbing the grant rather than drawing in marginal students. Pell grants thus bring new students to college exactly at the institutions where the multiplier is large and reduce enrollment at schools where it is small.

³³Appendix Figure A8 decomposes institutional spending into different categories. At non-profit schools, half of the additional spending after a rise in Pell grants goes to instruction, while research expenditures see the second-largest increase. At for-profit and two-year colleges, the figure shows that student services make up most of the higher expenditures.

Table 8: Pell Grant Multipliers by Institutional Composition

	Multiplier	Effect of Pell grants on		
	Income	Student loans	Tuition	Institution exp.
Baseline	2.715* (1.535)	1.975*** (0.444)	2.284** (0.969)	1.248** (0.537)
<i>Panel a: For-Profit vs. Non-Profit</i>				
All For-Profit	1.954 (1.592)	1.912** (0.864)	4.652*** (0.888)	1.257*** (0.226)
All Non-Profit	3.084* (1.848)	2.008*** (0.369)	1.280*** (0.492)	1.247* (0.704)
Difference (FP – NP)	-1.129 (2.007)	-0.095 (0.801)	3.372*** (0.836)	0.010 (0.593)
<i>Panel b: Two-Year vs. Four-Year</i>				
All Two-Year	-1.934 (4.128)	1.210*** (0.413)	1.067* (0.624)	0.350 (0.372)
All Four-Year	3.753** (1.512)	2.100*** (0.465)	2.469** (1.000)	1.393** (0.577)
Difference (2-Year – 4-Year)	-5.687 (3.852)	-0.889** (0.366)	-1.403** (0.604)	-1.043* (0.613)
<i>Panel c: Low-Quality vs. No Low-Quality (all institutions)</i>				
All Low-Quality	2.013 (2.066)	1.521*** (0.290)	1.783** (0.806)	0.651** (0.284)
No Low-Quality	3.220* (1.757)	2.302*** (0.575)	2.644** (1.108)	1.678** (0.691)
Difference (All – None)	-1.206 (2.197)	-0.781* (0.442)	-0.861 (0.583)	-1.027* (0.584)
<i>Panel d: Four-Year High-Quality vs. No Four-Year High-Quality</i>				
All 4y High-Quality	2.956* (1.728)	2.361*** (0.590)	2.712** (1.125)	1.716** (0.723)
No 4y High-Quality	2.587 (2.207)	1.486*** (0.282)	1.712** (0.821)	0.643** (0.290)
Difference (All – None)	0.369 (2.395)	0.875* (0.508)	0.999 (0.684)	1.073 (0.667)
<i>Panel e: Four-Year Low-Quality vs. No Four-Year Low-Quality</i>				
All 4y Low-Quality	3.109 (2.180)	1.649*** (0.334)	2.152** (0.938)	0.860** (0.364)
No 4y Low-Quality	2.518 (1.771)	2.100*** (0.538)	2.331** (1.025)	1.395** (0.670)
Difference (All – None)	0.591 (2.383)	-0.451 (0.538)	-0.179 (0.595)	-0.535 (0.685)
<i>Panel f: Postbaccalaureate vs. Undergraduate</i>				
All Postbac	-7.142 (10.707)	7.102*** (1.540)	-0.600 (2.856)	6.191** (2.807)
All Undergraduate	3.951** (1.849)	1.318*** (0.431)	2.650** (1.132)	0.612 (0.460)
Difference (Postbac – Undergraduate)	-11.093 (11.657)	5.784*** (1.586)	-3.250 (3.350)	5.579** (2.840)

Notes: Post-1999 sample: N=4,357. Compared to Table 2, samples change due to data availability of additional variables. Each panel evaluates the composition-weighted preferred post-1999 specification (S9) at the composition extremes (all local Pell dollars in the named group versus none); the Difference row is the change in each coefficient between the two rows. Income column is the Pell grant multiplier for real personal income per capita; the remaining columns are the effects of Pell grants on student loan disbursements, net tuition, and institution expenditures. Standard errors clustered by MSA in parentheses; *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

The findings in this section also speak to the so-called “Bennett Hypothesis,” named after the former U.S. Secretary of Education, who posited that colleges capture increases in student aid by raising tuition. Our results support a nuanced version of this hypothesis. Table 8 shows that tuition income does indeed rise when Pell grants expand, but only at for-profit colleges does this act as an implicit subsidy. At non-profit colleges, institutional spending rises in proportion to tuition income. The fact that Pell grants raise tuition therefore does not impede the beneficial local economic effects of expansions of the Pell Grant Program—the additional spending by colleges may even enhance them.

5.3. Heterogeneity by Timing

In the final exercise, we assess whether the average multiplier effect of Pell grants depends on whether the local economy is initially in an expansion or in a recession. During recessions the local economy is likely to have more idle resources, which means that there is more room for the rise in demand due to Pell grants to propagate locally before supply becomes a binding constraint. Previous evidence finds that, in general, fiscal spending indeed has greater effects on output when the economy is in recession.³⁴ If this holds for Pell grants too, they could form a particularly effective tool to stabilize macroeconomic activity.

To assess whether the Pell multiplier depends on the state of the local economy, we estimate equation (7) defining $z_{m,t-2}$ as the moving average of two-year lagged local income growth. Unlike school enrollment, income growth is not bounded between 0 and 1. We therefore follow Tenreyro and Thwaites (2016) by transforming income growth along the logistic function:

$$F(z_{m,t}) = \frac{\exp\left(\theta \frac{[z_{m,t} - \mu_m]}{\sigma_m}\right)}{1 + \exp\left(\theta \frac{[z_{m,t} - \mu_m]}{\sigma_m}\right)}, \quad (8)$$

where μ_m determines the fraction of the sample in which the metropolitan area is in recession, σ_m gives the standard deviation of biannual growth, and θ determines how stark the demarcation between recessions and expansions is (e.g., for a lower θ , the weight of observations is more evenly split between $\bar{\beta}$ and $\underline{\beta}$). μ_m is calibrated so that each area is in recession 20% of the sample period, which matches the percentage of quarters that the economy is in recession at the national level according to the NBER. We calibrate θ to 3 in line with Tenreyro and Thwaites (2016).

Results are presented in Table 9. The first column reproduces the average multiplier, the second column presents the multiplier in recessions, and the third column presents the multiplier in expansions. The recession (expansion) should be interpreted as the two-year effect of a relative increase in Pell grants on relative income growth if growth is initially at its *lowest* (highest) level in the dataset. Thus, the multipliers in the table are extrapolations for these extreme observations. We find that the multiplier of Pell grants is significantly higher during recessions than during expansions. At the extremes, we find

³⁴Examples include Auerbach and Gorodnichenko (2012), Corsetti et al. (2012), Ilzetzki et al. (2013), Blanchard and Leigh (2013), Jordà and Taylor (2016), and Berge et al. (2021). Ghassibe and Zanetti (2022) stress that the source of fluctuations matters as well. Demyanyk et al. (2019) find that during recessions the stimulative effects were concentrated in areas where households were financially constrained. Ramey and Zubairy (2018) do not find state-dependence in a historical sample on defense spending news shocks.

Table 9: State-Dependent Pell Grant Multipliers: Recessions vs. Expansions

	Multipliers		Effect of Pell grants on			Propagation
	Income	Employment	Student loans	Tuition	Institution exp.	share (%)
Baseline	2.715* (1.535)	2.668** (1.238)	1.975*** (0.444)	2.284** (0.969)	1.248** (0.537)	18.8%
Recession	6.002** (2.394)	3.449* (1.790)	0.732 (0.920)	1.770*** (0.611)	1.168*** (0.418)	70.0%
Expansion	0.950 (1.808)	1.851 (1.327)	2.670*** (0.376)	2.575** (1.189)	1.313* (0.689)	-157.3%
Diff. (Exp. – Rec.)	-5.052* (2.710)	-1.598 (1.857)	1.938** (0.892)	0.805 (0.720)	0.145 (0.584)	

Notes: Post-1999 sample: N=4,357. Compared to Table 2, samples change due to data availability of additional variables. Estimates follow specification (9) in Table 2. Baseline reports the aggregate Pell grant coefficients. Recession and Expansion report the coefficients on Pell grants interacted with $1 - F(z)$ and $F(z)$, respectively, where $F(z)$ is the logistic transition function of the twice-lagged moving average of local income growth (MSA-specific location at the 20th percentile and scale equal to the MSA standard deviation), with $F(z)$ included as a control and the Pell shift-share instrument interacted analogously; the state-dependence regressions use the trimming $\Delta y > -0.5$. The Difference row is the Expansion minus Recession contrast from the same regression. Income and Employment are the Pell grant multipliers for real personal income per capita and employment; the remaining coefficient columns are the effects of Pell grants on student loan disbursements, net tuition, and institution expenditures. The propagation share is the residual share of the income multiplier after the direct effect, student spending ($\lambda_s = 0.48$), and institution spending ($\lambda_i = 0.7$) components. Standard errors clustered by MSA in parentheses; *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

an income multiplier of 6.00 for recessions and 0.95 for expansions, while the respective employment multipliers are 3.45 in recessions and 1.85 in expansions.³⁵ Both expansion multipliers are furthermore not significantly different from zero at conventional significance levels. Turning to the other estimated elasticities, it is clear that the drivers of Pell grants' multiplier effect are different in recessions. First, the contribution of student spending to the multiplier is negligible: as students borrow less during recessions, the increase in their disposable income is approximately offset by higher tuition fees. Second, the institutional spending response does not change significantly. Third, there is a large difference in propagation: while less than 20% of the overall multiplier is driven by local propagation in the baseline estimation (for $\lambda_s = 0.48$), we find that 70% of the multiplier is driven by the residual propagation in recessions.

What drives the stronger recession-time propagation of Pell grants? Table 10 sheds light on this by decomposing the state-dependent employment multiplier into its two margins of adjustment: the unemployment rate and labor force participation. The contrast between states is stark: in recessions, entry from outside the labor force contributes 4.1 percentage points, or 82% of the recession multiplier, whereas in expansions the participation contribution is negative and the total effect is statistically indistinguishable from zero. The unemployment response, however, is similar in both states.

A plausible explanation for this pattern is that recessions raise the number of individuals whose labor force status is sensitive to the availability of jobs. [Elsby et al. \(2015\)](#) show that the labor force attachment of the unemployed rises during recessions, and that flows in and out of the labor force are sufficiently large to account for around one third of cyclical fluctuations in unemployment. An increase in labor

³⁵If we compare our Pell income multipliers for recessions with the recession multiplier calculated for the ARRA in [Chodorow-Reich \(2019\)](#) in his Table 1, we find that our multiplier estimates are statistically higher at the 5% significance level.

Table 10: State-Dependent Labor-Supply Decomposition of the Pell Grant Employment Multiplier

	Employment	Contribution of		Share of multiplier (%)	
	multiplier	Unemployment	Not in labor force	Unemployment	Not in labor force
Baseline	2.668** (1.238)	0.918 (0.753)	1.750 (1.151)	34.4%	65.6%
Recession	5.031*** (1.729)	0.918 (0.827)	4.113*** (1.554)	18.2%	81.8%
Expansion	-0.415 (1.803)	0.837 (1.186)	-1.252 (1.645)	-201.4%	301.4%
Diff. (Exp. – Rec.)	-5.446** (2.526)	-0.081 (1.321)	-5.365** (2.165)		

Notes: Post-1999 sample: N=4,357. Compared to Table 2, samples change due to data availability of additional variables. Employment multiplier is the Pell grant coefficient in the employment regression; the contributions of unemployment and of not-in-the-labor-force are the (sign-reversed) Pell grant coefficients in the corresponding unemployment-to-employment and inactives-to-employment growth regressions, so that they read as contributions to employment growth; shares express each contribution as a percentage of the employment multiplier. Baseline reports the aggregate Pell grant coefficients from the preferred specification. Recession and Expansion report the coefficients on Pell grants interacted with $1 - F(z)$ and $F(z)$, where $F(z)$ is the logistic transition function of the twice-lagged moving average of local employment growth, with $F(z)$ included as a control and the Pell shift-share instrument interacted analogously. The Difference row is the Expansion minus Recession contrast from the same regression. Standard errors clustered by MSA in parentheses; *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

demand during a recession therefore raises employment by drawing workers into the labor force and by retaining workers who would otherwise have left it. In expansions, few individuals outside the labor force are on the margin of entry, and an increase in demand generates little adjustment through participation.

6. Discussion

This paper estimates the effect of the Federal Pell Grant Program on short-run economic activity. Specifically, we assess how a relative increase in Pell grant disbursements at the metropolitan-area level raises the area's relative income and relative employment. To do so, we employ a shift-share approach where our identification relies on the variation in Pell grant receipts across metropolitan areas. We find that Pell grants have large effects on local economic activity: we estimate an income multiplier of 2.8 and an employment rate multiplier of 1.9 in the main specification. This implies that a 1% increase in Pell grants as a fraction of local income raises local income by 2.8% and local employment by 1.9%. A boom in local demand, driven by a combination of student spending, institutional spending, and local propagation, is responsible for these positive economic effects. The positive short-run effects of Pell grants are greater during recessions, and our overall estimate of the multiplier reflects the fraction of the time that the economy was in recession over our sample.

The multiplier of Pell grants on local income exceeds the median (1.9) and the average (2.1) of multipliers in previous studies that rely on geographic cross-sectional variation in other forms of fiscal spending, as surveyed by Chodorow-Reich (2019), as well as the 1.3 to 2.5 range for military spending in Nakamura and Steinsson (2014) and the 1.5 estimate for permanent fiscal transfers in Pennings (2021). Our estimates come with sizable standard errors, however, and we cannot reject that our preferred Pell grant

multiplier equals the multipliers in these studies at conventional significance levels. The imprecision partly reflects that the multiplier varies over the business cycle and across the institutions that recipients attend. The multiplier is higher at non-profit colleges: for-profit colleges raise tuition when Pell grants become more generous, but do not match the additional revenue with higher institutional spending. This offers some validation for recent restrictions on the Pell grant eligibility of students at for-profit institutions. Four-year institutions similarly have larger multipliers than two-year institutions. Higher-quality institutions also have higher multipliers than low-quality institutions. Pell grants are therefore particularly effective as a tool for countercyclical policy when awarded to students attending high-quality, four-year, non-profit colleges. Pell grant multipliers at 4-year colleges are, for example, significantly higher than most of the multipliers reported in [Nakamura and Steinsson \(2014\)](#) and [Pennings \(2021\)](#) at the 10% level.

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“The Fiscal Multiplier of Education Expenditures: Evidence from Pell Grants” — Online Appendix

A. Main Legislative Changes to the Pell Grant Program

Over time, the Pell Grant Program has undergone a number of legislative changes affecting eligibility, award amounts, and program funding. First, in 1978, the Middle Income Student Assistance Act (MISAA) expanded student eligibility by limiting the rate at which parental discretionary income was assessed under the EFC formula. This expansion broadened access to Pell grants for many middle-income families by reducing the extent to which parental resources affected measured financial need. The act was repealed two years later, in 1980. In 1990, the Omnibus Budget Reconciliation Act eliminated student aid eligibility at high-default schools, narrowing access to federal aid for students attending institutions with poor loan repayment outcomes. In 1992, the reauthorization of the Higher Education Act changed the definition of an independent student, thereby altering eligibility for some applicants whose aid status depended on family circumstances. In 1994, the Violent Crime Control and Law Enforcement Act eliminated Pell grants for prisoners.

A new period of expansion began in 2007, when Congress passed the College Cost Reduction and Access Act (CCRAA). The legislation supplemented grant funding and modified Pell eligibility by increasing the amount and types of income excluded from the EFC formula. Together with the countercyclical response of college enrollment during the Great Recession, a renewed set of legislative measures contributed to a substantial increase in Pell grant disbursements. These measures included the Higher Education Opportunity Act (HEOA) of 2008, which authorized year-round Pell grants and limited eligibility to 18 full-time semesters (or the equivalent); the American Recovery and Reinvestment Act (ARRA) of 2009, which provided additional funding to the Pell Grant Program and raised the maximum Pell grant by more than \$400; and the Health Care and Education Reconciliation Act of 2010, which increased the maximum Pell grant by more than \$600 and expanded eligibility by raising the income threshold for an automatic EFC of zero from \$20,000 to \$30,000.

Pell grant disbursements began to decline in 2011 as the economy recovered and undergraduate enrollment moved back toward pre-crisis levels.³⁶ At the same time, Congress reversed several earlier expansions. It eliminated the year-round Pell grant eligibility established in 2008, while also providing supplemental funding to the program and lowering the income threshold for an automatic EFC of zero to \$23,000. In 2012, the Consolidated Appropriations Act provided additional funding to the Pell Grant Program and further tightened program parameters by reducing lifetime Pell eligibility to 12 semesters. Overall, the period from the late 2000s through the early 2010s was characterized by a combination of temporary expansions in generosity and eligibility followed by measures aimed at containing program costs and limiting long-term eligibility.

³⁶During economic recovery, fewer individuals qualify to receive Pell grants. Enrollment is counter-cyclical, as some individuals choose employment rather than education when labor market opportunities improve.

B. Additional Tables and Figures on Shift-Share Validity Tests

Table A1: Balance Test for the Specification with Fiscal Transfers

	Full Sample				Post 1999			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Shares	Income	Growth	Empl. Growth	Shares	Income	Growth	Empl. Growth
	Pass	Pass	Pass	Pass	Pass	Pass	Pass	Pass
Spending Growth	0.006 (0.014)	-0.013 (0.023)	-0.038** (0.017)	✓	-0.005 (0.008)	-0.022 (0.033)	-0.048** (0.019)	✓
Log(Tuition)	-0.006 (0.048)	-0.068 (0.051)	-0.019 (0.050)	✓	-0.022 (0.049)	-0.237*** (0.063)	-0.118* (0.064)	✓
D.Log(Students)	0.017 (0.010)	0.001 (0.009)	-0.007 (0.009)	✓	0.021* (0.011)	0.001 (0.011)	-0.009 (0.007)	✓
For Profit	0.052 (0.035)	-0.002 (0.040)	0.026 (0.028)	✓	0.006 (0.028)	0.027 (0.057)	0.092* (0.047)	✓
Share Black	0.157 (0.233)	-0.026 (0.159)	0.246 (0.158)	✓	0.593 (0.388)	0.046 (0.524)	0.427 (0.360)	✓
Share Hisp.	-0.172 (0.142)	0.558*** (0.144)	0.780*** (0.117)	✓	-0.287 (0.223)	0.353 (0.363)	0.794*** (0.278)	✓
Share Bach.	-0.115 (0.133)	0.074 (0.102)	0.271*** (0.081)	✓	0.026 (0.134)	0.654*** (0.166)	0.771*** (0.147)	✓
Risk Score					-0.041 (0.029)	-0.022 (0.042)	-0.090* (0.048)	✓
Age					0.002 (0.037)	-0.016 (0.041)	0.001 (0.038)	✓
Debt to Income					-0.052 (0.032)	0.238 (0.191)	-0.082* (0.044)	✓
Card Util.					0.026* (0.016)	-0.037 (0.030)	-0.014 (0.024)	✓
Mort. Delinq.					0.013 (0.015)	-0.009 (0.019)	-0.022 (0.019)	✓
Observations	8,436	8,436	8,436		4,447	4,447	4,447	
R-square	0.7	0.3	0.5		0.8	0.4	0.6	
Time FE	Yes	Yes	Yes		Yes	Yes	Yes	
MSA FE	Yes	Yes	Yes		Yes	Yes	Yes	

Notes: Independent variables are twice lagged in columns (2), (3), (6), and (7) except for spending growth. Spending Growth refers to the change in the total amount of fiscal transfers due to state appropriations, SNAP, UI, and HUD programs. MSA-clustered standard errors are in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A2: Balance Test for Appropriations

	Full Sample				Post 1999			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Shares	Income Growth	Empl. Growth	Pass	Shares	Income Growth	Empl. Growth	Pass
Log(Tuition)	-0.015 (0.028)	-0.069 (0.051)	-0.021 (0.050)	✓	-0.037 (0.034)	-0.239*** (0.063)	-0.123* (0.064)	✓
D.Log(Students)	-0.000 (0.003)	0.001 (0.009)	-0.006 (0.009)	✓	-0.003 (0.006)	0.001 (0.011)	-0.009 (0.007)	✓
For Profit	0.026 (0.018)	-0.002 (0.040)	0.026 (0.028)	✓	0.030** (0.014)	0.029 (0.057)	0.097** (0.048)	×
Share Black	0.183 (0.125)	-0.027 (0.159)	0.244 (0.158)	✓	-0.147 (0.156)	0.044 (0.523)	0.423 (0.359)	✓
Share Hisp.	0.347*** (0.076)	0.557*** (0.143)	0.777*** (0.117)	×	0.470*** (0.148)	0.349 (0.362)	0.787*** (0.279)	×
Share Bach.	0.271*** (0.067)	0.075 (0.101)	0.274*** (0.081)	×	0.342*** (0.082)	0.662*** (0.164)	0.788*** (0.146)	×
Risk Score					0.007 (0.017)	-0.024 (0.043)	-0.093* (0.048)	✓
Age					0.015 (0.020)	-0.014 (0.041)	0.005 (0.038)	✓
Debt to Income					0.005 (0.017)	0.237 (0.191)	-0.084* (0.045)	✓
Card Util.					-0.014 (0.010)	-0.038 (0.030)	-0.015 (0.024)	✓
Mort. Delinq.					0.003 (0.005)	-0.009 (0.019)	-0.022 (0.019)	✓
Observations	8,436	8,436	8,436		4,447	4,447	4,447	
R-square	1.0	0.3	0.5		1.0	0.4	0.6	
Time FE	Yes	Yes	Yes		Yes	Yes	Yes	
MSA FE	Yes	Yes	Yes		Yes	Yes	Yes	

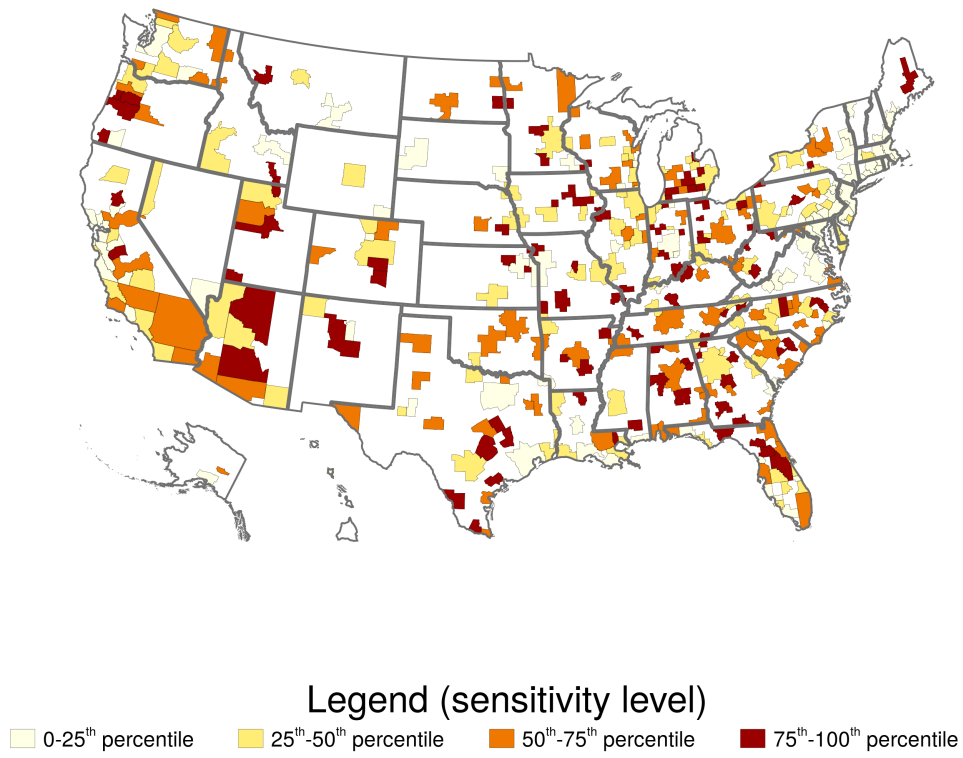
Notes: Independent variables are twice lagged in columns (2), (3), (6), and (7). MSA-clustered standard errors are in parentheses.
*, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A3: Balance Test for Fiscal Transfers

	Post 1999				Full Cases Only			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Shares	Income Growth	Empl. Growth	Pass	Shares	Income Growth	Empl. Growth	Pass
Log(Tuition)	-0.019 (0.035)	-0.239*** (0.063)	-0.123* (0.064)	✓	-0.003 (0.041)	-0.121 (0.125)	-0.066 (0.109)	✓
D.Log(Students)	0.000 (0.006)	0.001 (0.011)	-0.009 (0.007)	✓	0.009 (0.013)	-0.009 (0.028)	-0.022 (0.014)	✓
For Profit	0.038** (0.015)	0.029 (0.057)	0.097** (0.048)	×	-0.000 (0.020)	0.030 (0.079)	0.119* (0.067)	✓
Share Black	-0.020 (0.182)	0.044 (0.523)	0.423 (0.359)	✓	-0.209 (0.256)	0.971 (1.384)	1.050 (1.004)	✓
Share Hisp.	0.541*** (0.166)	0.349 (0.362)	0.787*** (0.279)	×	0.739*** (0.259)	0.245 (0.714)	0.834 (0.643)	✓
Share Bach.	0.407*** (0.083)	0.662*** (0.164)	0.788*** (0.146)	×	0.349*** (0.112)	0.578 (0.389)	0.698** (0.281)	×
Risk Score	0.013 (0.019)	-0.024 (0.043)	-0.093* (0.048)	✓	0.014 (0.022)	-0.115** (0.053)	-0.107* (0.061)	✓
Age	0.041* (0.022)	-0.014 (0.041)	0.005 (0.038)	✓	0.054* (0.028)	-0.040 (0.051)	0.050 (0.046)	✓
Debt to Income	0.017 (0.016)	0.237 (0.191)	-0.084* (0.045)	✓	0.041** (0.018)	0.527 (0.338)	-0.143* (0.083)	×
Card Util.	-0.012 (0.011)	-0.038 (0.030)	-0.015 (0.024)	✓	-0.009 (0.009)	-0.067 (0.041)	0.019 (0.028)	✓
Mort. Delinq.	0.019*** (0.006)	-0.009 (0.019)	-0.022 (0.019)	✓	0.022*** (0.007)	0.024 (0.028)	-0.000 (0.027)	✓
Observations	4,447	4,447	4,447		2,523	2,523	2,523	
R-square	1.0	0.4	0.6		1.0	0.5	0.7	
Time FE	Yes	Yes	Yes		Yes	Yes	Yes	
MSA FE	Yes	Yes	Yes		Yes	Yes	Yes	

Notes: Independent variables are twice lagged in columns (2), (3), (6), and (7). Fiscal Transfers refers to the total amount of fiscal transfers due to state appropriations, SNAP, UI, and HUD programs. MSA-clustered standard errors are in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Figure A1. Geographical Distribution of the Ratio of Pell Grant Recipients over Population



Notes: The figure plots $s_{m,2010}$ from equation (3). Blank areas fall outside metropolitan areas or, in rare cases, are areas that never receive Pell grants.

Table A4: Effect of Pell Grants on Local Income Per Capita Controlling for Lagged Pell Grants

	Full Sample				Post 1999						Full Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	2SLS	2SLS	2SLS	GMM	2SLS	2SLS	2SLS	2SLS	2SLS	GMM	OLS
<i>Panel A: Income Growth</i>											
Multiplier _t	4.125**	4.149**	3.851**	4.482**	4.813**	4.849**	4.194**	4.233**	3.938*	5.561**	-1.922**
	(1.913)	(1.910)	(1.883)	(1.970)	(2.195)	(2.190)	(2.078)	(2.059)	(2.041)	(2.315)	(0.920)
Multiplier _{t-1}	-1.789	-1.967	-1.393	-2.513	-1.514	-1.637	-1.504	-1.680	-1.026	-3.421	0.460
	(1.819)	(1.847)	(1.788)	(1.868)	(1.992)	(2.046)	(1.968)	(2.009)	(1.973)	(2.468)	(0.859)
<i>Panel B: Employment Growth</i>											
Multiplier _t	2.448*	2.407**	2.055*	3.167**	3.201**	3.124**	2.500*	2.444*	2.539*	3.372**	-2.586***
	(1.262)	(1.207)	(1.181)	(1.346)	(1.496)	(1.429)	(1.331)	(1.283)	(1.345)	(1.478)	(0.912)
Multiplier _{t-1}	-1.065	-0.753	-0.166	-0.951	-0.105	0.158	0.471	0.434	0.126	-1.846	1.883***
	(1.166)	(1.115)	(1.085)	(1.122)	(1.256)	(1.268)	(1.222)	(1.235)	(1.235)	(1.405)	(0.618)
Observations	8,436	8,436	8,436	8,436	4,447	4,447	4,447	4,447	4,447	4,447	8,436
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Approp.		Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes
MSA Controls			Yes	Yes			Yes	Yes	Yes	Yes	Yes
Financial Controls								Yes	Yes	Yes	
Fiscal Transfers									Yes	Yes	
Δ Pell Grants F-test	74.8	76.3	76.2	42.7	52.5	54.7	53.6	54.1	53.2	28.1	-
Joint F-test	35.3	35.8	35.9	301.1	24.5	21.2	20.9	20.8	29.8	18.4	-

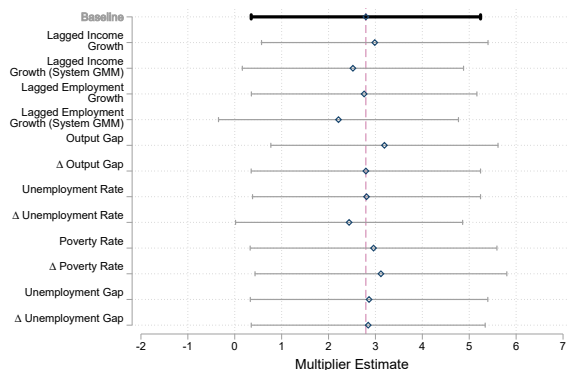
Notes: 2SLS estimates use the SSIV strategy described in eq. (3). SSIV strategies for lagged Pell grants, state appropriations, and wider fiscal transfers are analogous. MSA controls: change in undergraduate students (log) in the last 2 years, average tuition fee (log), for-profit penetration, percentage of population black, percentage Hispanic, percentage with at least a bachelor's degree. Data on financial controls is from Federal Reserve Bank of New York/Equifax Consumer Credit Panel (available after 1999): median Equifax Risk Score and age, debt-to-income ratio, credit card utilization, and 30-day mortgage delinquency rate. Fiscal Transfers: sum of transfers due to state appropriations, SNAP, UI, and HUD programs. Δ Pell Grants F-test is the robust F-statistic of the first-stage regression of Pell grants. Joint F-test is the robust F-statistic of the joint IV set. The GMM columns' Joint F-test is the auxiliary robust first-stage F-statistic for fiscal transfers. MSA-clustered standard errors are in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A5: Effect of Pell Grants on Local Income Per Capita Using Shares Directly

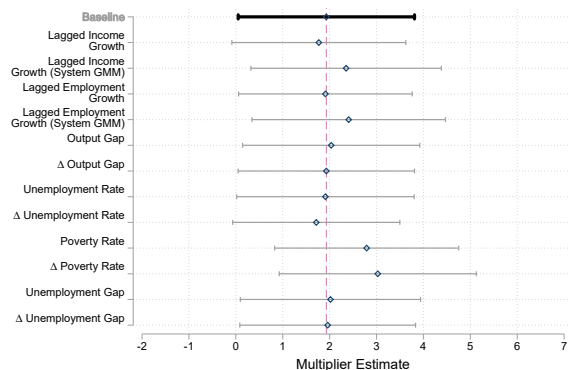
	Full Sample				Post 1999						Full Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	2SLS	2SLS	2SLS	GMM	2SLS	2SLS	2SLS	2SLS	2SLS	GMM	OLS
<i>Panel A: Income Growth</i>											
Multiplier	1.762 (1.308)	1.890 (1.348)	2.103 (1.338)	1.457 (1.676)	4.418** (1.726)	4.351** (1.759)	3.811** (1.699)	3.769** (1.698)	3.852** (1.668)	3.789*** (1.380)	-1.672* (0.925)
<i>Panel B: Employment Growth</i>											
Multiplier	0.325 (0.978)	1.022 (0.986)	1.144 (0.988)	2.264 (1.627)	2.707** (1.236)	2.888** (1.260)	2.465** (1.220)	2.354** (1.194)	2.228* (1.208)	2.240** (1.136)	-1.559** (0.772)
Observations	8,436	8,436	8,436	8,436	4,447	4,447	4,447	4,447	4,447	4,447	8,436
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Approp.		Yes	Yes	Yes		Yes	Yes	Yes			Yes
MSA Controls			Yes	Yes			Yes	Yes	Yes	Yes	Yes
Financial Controls								Yes	Yes	Yes	
Fiscal Transfers									Yes	Yes	
Δ Pell Grants F-test	107.6	72.3	71.3	107.0	12.8	13.6	13.3	13.1	14.7	12.8	-
Joint F-test	107.6	17.8	17.9	136.6	12.8	8.3	8.4	8.5	10.2	116.9	-

Notes: SSIV strategy for the Pell grants regressor uses the twice-lagged share of recipients in MSA population directly as instruments. SSIV strategies for state appropriations and wider fiscal transfers use the approach in eq. (3). MSA controls: change in undergraduate students (log) in the last 2 years, average tuition fee (log), for-profit penetration, percentage of population black, percentage Hispanic, percentage with at least a bachelor's degree. Data on financial controls is from Federal Reserve Bank of New York/Equifax Consumer Credit Panel (available after 1999): median Equifax Risk Score and age, debt-to-income ratio, credit card utilization, and 30-day mortgage delinquency rate. Fiscal Transfers: sum of transfers due to state appropriations, SNAP, UI, and HUD programs. Δ Pell Grants F-test is the robust F-statistic of the first-stage regression of Pell grants. Joint F-test is the robust F-statistic of the joint IV set. The GMM columns' Joint F-test is the auxiliary robust first-stage F-statistic for fiscal transfers. MSA-clustered standard errors are in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

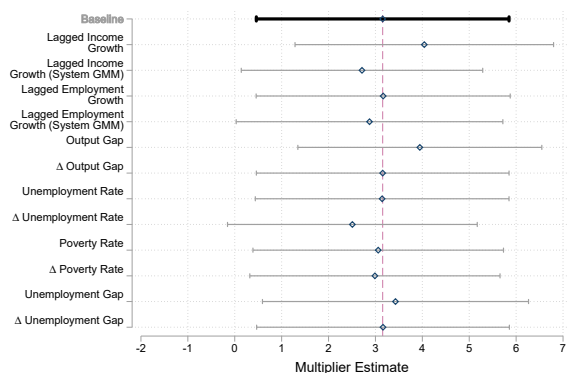
Figure A2. Multiplier Estimates: Robustness Checks for Mean Reversion and Lower-Tail Dynamics



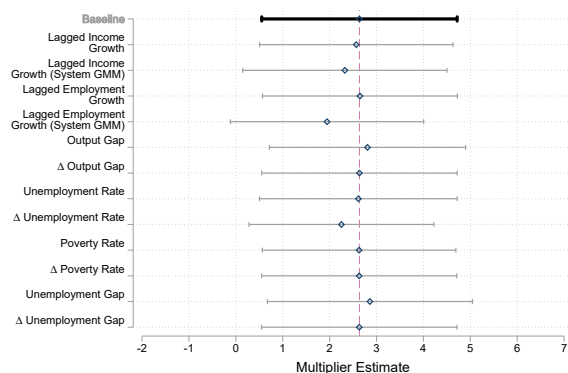
(a) Income - Full Sample



(b) Employment - Full Sample



(c) Income - Post 1999 Sample



(d) Employment - Post 1999 Sample

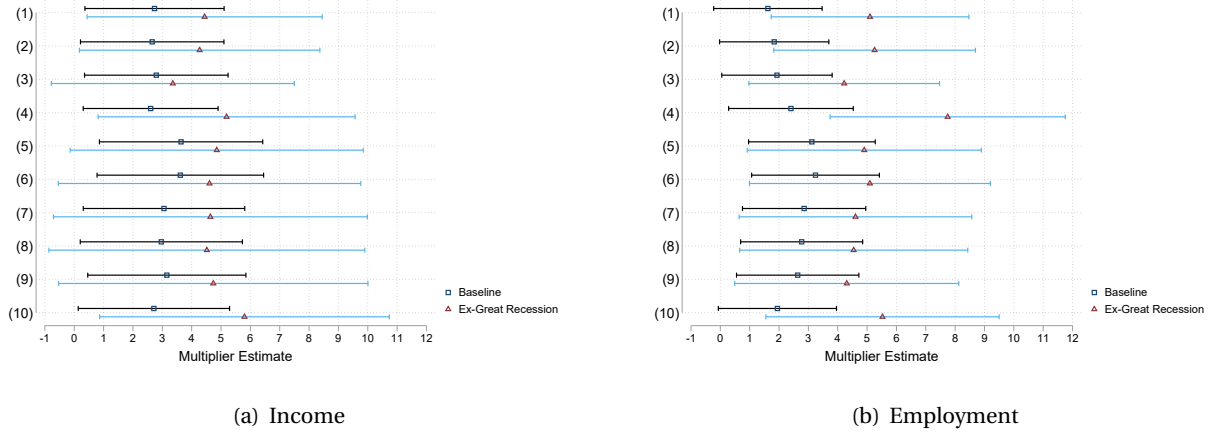
Notes: The figures plot the multiplier estimate (scatter) and 90% confidence bounds based on clustered standard errors. We construct an output gap by applying a standard Hodrick–Prescott filter to local personal income and defining the gap as the difference between current and trend log income. The unemployment gap is the deviation of unemployment from its HP trend (the “unemployment gap”). Local poverty rates are obtained from the Census Bureau’s Small Area Income and Poverty Estimates (SAIPE) database, which provides county-level estimates of the share of the population in poverty according to the Official Poverty Measure (OPM), using a combination of data from the Current Population Survey (CPS), IRS income tax records, and food stamp participation. Adding this control, either in levels or first differences, raises multiplier estimates by 0–0.1 points for income and by up to 1.0 point for employment.

Table A6: Effect of Pell Grants on Local Income Per Capita and Employment (Last Growth Period)

	Full Sample				Post 1999						Full Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	2SLS	2SLS	2SLS	GMM	2SLS	2SLS	2SLS	2SLS	2SLS	GMM	OLS
<i>Panel A: Income Growth</i>											
Multiplier	3.064** (1.449)	2.982** (1.497)	3.178** (1.492)	3.086** (1.495)	3.903** (1.696)	3.881** (1.735)	3.235* (1.669)	3.140* (1.669)	3.361** (1.626)	3.473** (1.650)	-1.621* (0.922)
<i>Panel B: Employment Growth</i>											
Multiplier	1.697 (1.156)	1.928* (1.169)	2.050* (1.179)	2.749** (1.352)	3.212** (1.359)	3.351** (1.377)	2.889** (1.314)	2.774** (1.293)	2.623** (1.294)	2.092* (1.255)	-1.588** (0.774)
Observations	8,429	8,429	8,429	8,429	4,446	4,446	4,446	4,446	4,446	4,446	8,429
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Approp.		Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes
MSA Controls			Yes	Yes			Yes	Yes	Yes	Yes	Yes
Financial Controls								Yes	Yes	Yes	
Fiscal Transfers									Yes	Yes	
Δ Pell Grants F-test	98.3	90.8	91.2	52.7	70.7	67.0	66.9	67.3	74.0	50.2	-
Joint F-test	-	66.4	66.9	300.4	-	36.5	36.3	36.3	51.8	23.5	-

Notes: SSIV strategy for the Pell grants regressor uses the share of recipients in MSA population from the most recent period prior to t-2 in which the local economy experienced positive growth (see eq. 3). SSIV strategies for state appropriations and wider fiscal transfers use the approach in eq. (3). MSA controls: change in undergraduate students (log) in the last 2 years, average tuition fee (log), for-profit penetration, percentage of population black, percentage Hispanic, percentage with at least a bachelor's degree. Data on financial controls is from Federal Reserve Bank of New York/Equifax Consumer Credit Panel (available after 1999): median Equifax Risk Score and age, debt-to-income ratio, credit card utilization, and 30-day mortgage delinquency rate. Fiscal Transfers: sum of transfers due to state appropriations, SNAP, UI, and HUD programs. Δ Pell Grants F-test is the robust F-statistic of the first-stage regression of Pell grants. Joint F-test is the robust F-statistic of the joint IV set. The GMM columns' Joint F-test is the auxiliary robust first-stage F-statistic for fiscal transfers. MSA-clustered standard errors are in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Figure A3. Multiplier Estimates: Effect of Excluding the Great Recession



Notes: The figures plot the multiplier estimate (scatter) and 90% confidence bounds based on clustered standard errors. Numbers refer to the specification number in the tables and determine the set of controls that are included in the analysis. Column (1): time and MSA fixed effects. Column (2): time and MSA fixed effects and state appropriation controls. Column (3): time and MSA fixed effects, state appropriation controls, and broader MSA controls. Column (4): time and MSA fixed effects, state appropriation controls, and broader MSA controls and a lagged dependent variable. Columns (5), (6), (7) respectively add the same sequence of controls for the post 1999 sample. Columns (8) and (9) add financial controls and fiscal transfers, Column (10) adds a lagged dependent variable.

C. Additional Tables and Figures

Table A7: Pell Grants Multiplier Weighting by Two-Year Lagged MSA Population Logarithm

	Full Sample				Post 1999						Full Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	2SLS	2SLS	2SLS	GMM	2SLS	2SLS	2SLS	2SLS	2SLS	GMM	OLS
<i>Panel A: Income Growth</i>											
Multiplier	2.898** (1.434)	2.816* (1.471)	2.940** (1.479)	2.608* (1.420)	3.723** (1.690)	3.708** (1.719)	3.175* (1.667)	3.080* (1.662)	3.248** (1.626)	2.777* (1.560)	-1.662* (0.923)
<i>Panel B: Employment Growth</i>											
Multiplier	1.711 (1.091)	1.895* (1.103)	1.978* (1.115)	2.432* (1.258)	3.191** (1.292)	3.302** (1.304)	2.935** (1.256)	2.850** (1.239)	2.728** (1.240)	1.664 (1.159)	-1.577** (0.761)
Observations	8,436	8,436	8,436	8,436	4,447	4,447	4,447	4,447	4,447	4,447	8,436
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Approp.		Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes
MSA Controls			Yes	Yes			Yes	Yes	Yes	Yes	Yes
Financial Controls								Yes	Yes	Yes	
Fiscal Transfers									Yes	Yes	
Δ Pell Grants F-test	103.4	95.8	95.9	50.2	74.8	71.3	70.8	70.7	77.7	44.3	-
Joint F-test	-	70.1	70.4	359.2	-	38.7	38.4	38.3	54.1	173.3	-

Notes: 2SLS estimates use the SSIV strategy described in eq. (3). SSIV strategies for state appropriations and wider fiscal transfers are analogous. In the GMM specifications, variables are multiplied by the square root of the weight before estimation. MSA controls: change in undergraduate students (log) in the last 2 years, average tuition fee (log), for-profit penetration, percentage of population black, percentage Hispanic, percentage with at least a bachelor's degree. Data on financial controls is from Federal Reserve Bank of New York/Equifax Consumer Credit Panel (available after 1999): median Equifax Risk Score and age, debt-to-income ratio, credit card utilization, and 30-day mortgage delinquency rate. Fiscal Transfers: sum of transfers due to state appropriations, SNAP, UI, and HUD programs. Δ Pell Grants F-test is the robust F-statistic of the first-stage regression of Pell grants. Joint F-test is the robust F-statistic of the joint IV set. The GMM columns' Joint F-test is the auxiliary robust first-stage F-statistic for fiscal transfers. MSA-clustered standard errors are in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A8: Effect of Pell Grants on Local Income Per Capita and Employment - Max Pell IV

	Full Sample				Post 1999						Full Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	2SLS	2SLS	2SLS	GMM	2SLS	2SLS	2SLS	2SLS	2SLS	GMM	OLS
<i>Income Growth</i>											
Multiplier	2.849*	2.793*	2.881*	2.742*	4.080**	4.061**	3.526**	3.455*	3.664**	3.172*	-1.672*
	(1.575)	(1.618)	(1.618)	(1.531)	(1.770)	(1.807)	(1.767)	(1.773)	(1.735)	(1.666)	(0.925)
<i>Employment Growth</i>											
Multiplier	1.920	2.080*	2.119*	2.911**	3.388**	3.537**	3.141**	3.053**	2.892**	2.181*	-1.559**
	(1.233)	(1.242)	(1.250)	(1.448)	(1.357)	(1.381)	(1.332)	(1.317)	(1.310)	(1.261)	(0.772)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Approp.		Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes
MSA Controls			Yes	Yes			Yes	Yes	Yes	Yes	Yes
Financial Controls								Yes	Yes	Yes	
Fiscal Transfers									Yes	Yes	
Δ Pell Grants F-test	94.0	88.4	88.5	54.9	75.5	71.4	70.6	70.4	77.9	46.5	-
Joint F-test	-	65.4	65.8	74.0	-	37.0	36.6	36.5	52.1	58.3	-
Observations	8,436	8,436	8,436	8,436	4,447	4,447	4,447	4,447	4,447	4,447	8,436

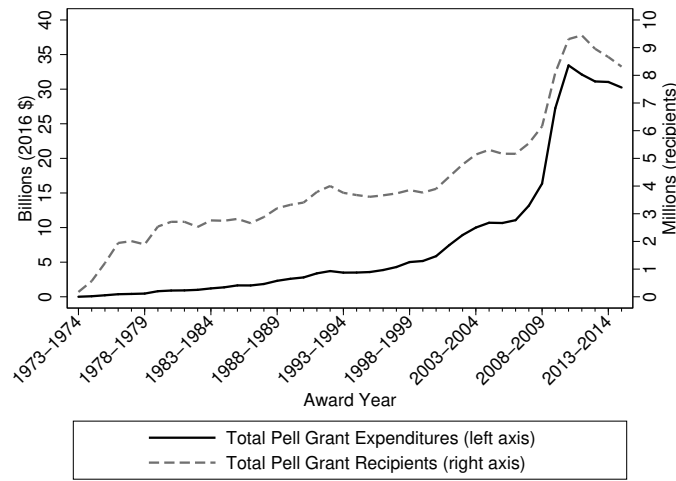
2SLS estimates use the SSIV strategy described in eq. (3) with changes in the maximum Pell grant as shocks. SSIV strategies for state appropriations and wider fiscal transfers use the approach described in eq. (3). MSA controls: change in undergraduate students (log) in the last 2 years, average tuition fee (log), for-profit penetration, percentage of population black, percentage Hispanic, percentage with at least a bachelor's degree. Data on financial controls is from Federal Reserve Bank of New York/Equifax Consumer Credit Panel (available after 1999): median Equifax Risk Score and age, debt-to-income ratio, credit card utilization, and 30-day mortgage delinquency rate. Fiscal Transfers: sum of transfers due to state appropriations, SNAP, UI, and HUD programs. Δ Pell Grants F-test is the robust F-statistic of the first-stage regression of Pell grants. Joint F-test is the robust F-statistic of the joint IV set. The GMM columns' Joint F-test is the auxiliary robust first-stage F-statistic for fiscal transfers. MSA-clustered standard errors are in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A9: Decomposition of the Pell Employment Multiplier into Race/Ethnicity-Specific Employment

<i>Race Group</i>	Full Sample		Post 1999	
	Employment	Percentage of Total	Employment	Percentage of Total
White	2.421** (1.187)	91.5%	2.709** (1.179)	103.1%
Black or African American	-0.410 (0.505)	-15.5%	-0.524 (0.511)	-19.9%
American Indian or Alaska Native	0.083 (0.067)	3.1%	0.056 (0.060)	2.1%
Asian	0.390*** (0.145)	14.7%	0.286** (0.126)	10.9%
Native Hawaiian or Pacific Isl.	0.042* (0.023)	1.6%	0.028** (0.011)	1.1%
Two or More Race Groups	0.120** (0.054)	4.5%	0.073* (0.041)	2.8%
Total	2.646** (1.263)	100%	2.628** (1.270)	100%

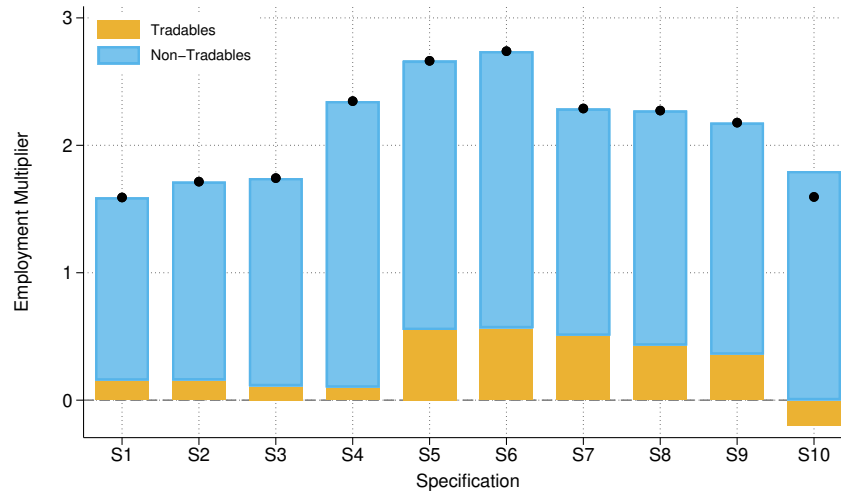
Notes: Full sample: N=5,944; post-1999 sample: N=4,352. Samples change due to QWI data availability. Each row presents the Race/Ethnicity-specific employment multiplier estimate β^a . 2SLS estimates follow (4). Controls follow the preferred specifications (3) and (9) from Table 2. Clustered standard errors in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Figure A4. The Pell Grant Program: Expenditures and Recipients



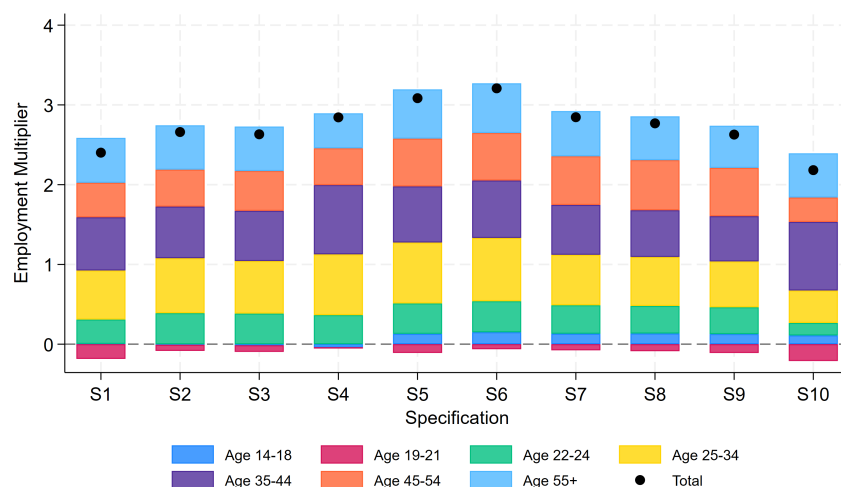
Notes: The figure plots the total value of Pell grants in 2016 USD. Data is obtained from the Title IV Program Volume Reports by the Department of Education.

Figure A5. Decomposition of the Pell Employment Multiplier into Sectors



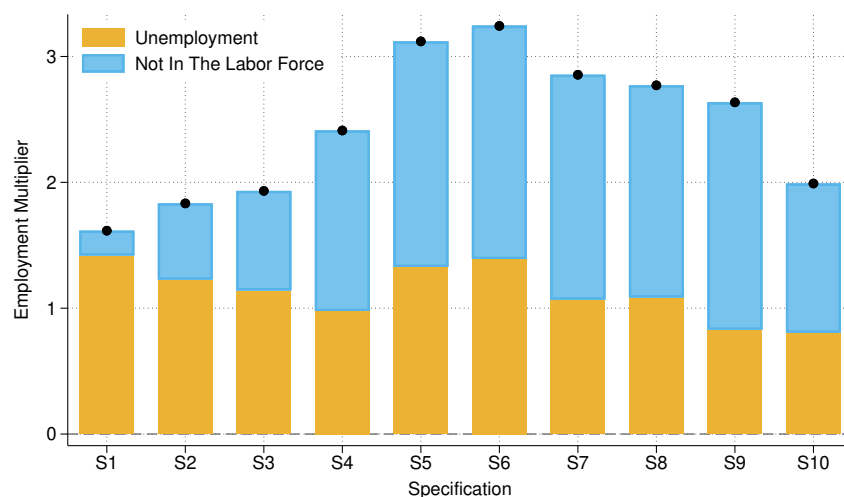
Notes: The figures plot the multiplier estimate (scatter) and contributions of sectors. Numbers refer to the specification number in the tables and determine the set of controls that are included in the analysis. Column (1): time and MSA fixed effects. Column (2): time and MSA fixed effects and state appropriation controls. Column (3): time and MSA fixed effects, state appropriation controls, and broader MSA controls. Column (4): time and MSA fixed effects, state appropriation controls, and broader MSA controls and a lagged dependent variable. Columns (5), (6), (7) respectively add the same sequence of controls for the post 1999 sample. Columns (8) and (9) add financial controls and fiscal transfers, Column (10) adds a lagged dependent variable. In Columns (4) and (10), we use a single-step weight matrix so that coefficients add up.

Figure A6. Decomposition of the Pell Employment Multiplier into Age-Specific Employment



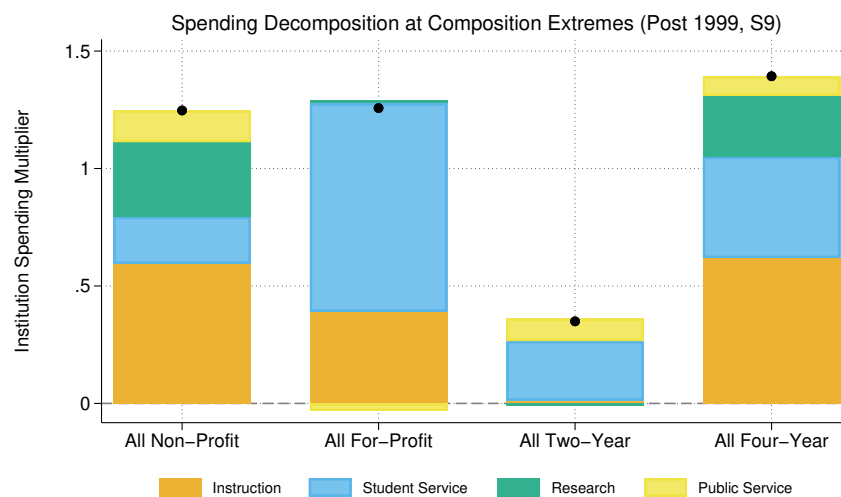
Notes: The figures plot the multiplier estimate (scatter) and contributions of each age group. Numbers refer to the specification number in the tables and determine the set of controls that are included in the analysis. Column (1): time and MSA fixed effects. Column (2): time and MSA fixed effects and state appropriation controls. Column (3): time and MSA fixed effects, state appropriation controls, and broader MSA controls. Column (4): time and MSA fixed effects, state appropriation controls, and broader MSA controls and a lagged dependent variable. Columns (5), (6), (7) respectively add the same sequence of controls for the post 1999 sample. Columns (8) and (9) add financial controls and fiscal transfers, Column (10) adds a lagged dependent variable. In Columns (4) and (10), we use a single-step weight matrix so that coefficients add up.

Figure A7. Decomposition of the Pell Employment Multiplier into Unemployment and Participation



Notes: The figures plot the multiplier estimate (scatter) and contributions of unemployment and labor force participation. Numbers refer to the specification number in the tables and determine the set of controls that are included in the analysis. Column (1): time and MSA fixed effects. Column (2): time and MSA fixed effects and state appropriation controls. Column (3): time and MSA fixed effects, state appropriation controls, and broader MSA controls. Column (4): time and MSA fixed effects, state appropriation controls, and broader MSA controls and a lagged dependent variable. Columns (5), (6), (7) respectively add the same sequence of controls for the post 1999 sample. Columns (8) and (9) add financial controls and fiscal transfers, Column (10) adds a lagged dependent variable. In Columns (4) and (10), we use a single-step weight matrix so that coefficients add up.

Figure A8. Decomposition of the Pell Institutional Spending Multiplier into Categories of Spending



Notes: The figures plot the multiplier estimate (scatter) for institutional spending and contributions of different types of spending. The estimates use the set of controls for the preferred specification for the post 1999 sample (column 9).

D. Regressions using School-Level Micro Data

This appendix estimates the effect of Pell grants on school-level outcomes using a series of shift-share instrumental variable regressions analogous to the MSA-level regressions in the main text. We use school-level micro data from the Delta Cost Project (see Section 2.3). We estimate the response of outcome $S_{i,t}$ at educational institution i to a change in Pell grant disbursements to the school's students, $e_{i,t}$, using the following estimation equation:

$$\frac{\Delta S_{i,t;t-2}}{S_{i,t-2}} = \beta \left(\frac{\Delta e_{i,t;t-2}}{\bar{y}_{i,t-2}} \right) + \phi_i + \psi_t + \gamma' x_{i,t-2} + \mu_{i,t}, \quad (9)$$

where $\bar{y}_{i,t-2}$ denotes the school's lagged tuition receipts. The normalization of Pell grant growth by lagged tuition receipts is analogous to the normalization by lagged local income in the MSA-level regressions. Equation (9) therefore mirrors the structure of equation (2) in the main text, with the school replacing the metropolitan area as the unit of observation: β measures the growth of the outcome caused by an increase in Pell grants equal to 1% of the school's tuition receipts, just as the multiplier measures the income response to an increase in Pell grants equal to 1% of local income.

The outcomes we consider are applications, enrollment, and the share of minority students in total enrollment. We instrument Pell grant growth with a shift-share instrument constructed as in the MSA-level analysis, $b_{i,t} = \Delta \bar{e}_{t;t-2} \cdot s_{i,t-2}$, where $\Delta \bar{e}_{t;t-2}$ is the national change in the average Pell grant per full-time equivalent student and $s_{i,t-2}$ is the lagged share of Pell grant recipients in the school's enrollment (in full-time equivalents). As at the MSA level, identification thus rests on the interaction of national changes in program generosity with predetermined local exposure shares.

Table A10 presents the results for applications, enrollment, and the share of minority students in total enrollment, respectively. Each panel splits the sample into for-profit institutions (columns 1–4) and non-profit institutions (columns 5–8). Within each panel, the first column presents the baseline specification with school and time fixed effects, the second column adds controls to match the preferred specifications in the main text, and the remaining columns interact Pell grant growth with indicators for above-median quality (HQ) and two-year (2-Y) institutions, as in the main text. Across the three outcomes, a consistent pattern emerges: at non-profit institutions, an increase in Pell grants raises applications, enrollment, and the share of minority students, whereas at for-profit institutions the application response is absent and the enrollment and composition responses are negative.³⁷ The interactions show that the positive non-profit enrollment response is concentrated at four-year institutions and larger at above-median-quality schools. We discuss the interpretation of these results in Section 5.

³⁷The negative for-profit enrollment coefficient is consistent with these schools raising tuition rather than expanding enrollment when Pell generosity increases (Table 8, Panel a). It is not in tension with the positive MSA-level results: the coefficient measures enrollment at the institution itself, and students priced out of for-profit colleges may enroll elsewhere in the same metropolitan area (Goodman and Volz, 2020). Part of the estimate may also reflect the contraction of the for-profit sector after 2010, which was concentrated at high-Pell-share institutions.

Table A10: Effects of Pell Grants on Applications and Enrollment: School-Level Evidence

	For-Profit				Non-Profit			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Applications</i>								
Δ Pell Grants (% Tuition)	-0.087 (0.889)	-0.188 (0.934)	0.164 (1.064)	-0.643 (1.003)	0.700*** (0.250)	0.702*** (0.250)	0.695*** (0.249)	0.637** (0.258)
HQ $\times$ Δ Pell Grants (% Tuition)			-1.672 (1.248)	-3.131 (4.678)			0.316 (0.401)	0.400 (0.384)
2-Y $\times$ Δ Pell Grants (% Tuition)				1.612 (1.324)				0.238 (0.355)
HQ $\times$ 2-Y $\times$ Δ Pell Grants (% T.)				0.516 (4.455)				-0.271 (0.761)
Observations	3,232	3,232	3,232	3,232	14,972	14,972	14,972	14,972
Instrument F-test	12.9	12.5	33.9	75.0	113.1	114.0	103.1	117.0
<i>Panel B: Total Enrollment</i>								
Δ Pell Grants (% Tuition)	-2.730*** (0.253)	-2.726*** (0.253)	-2.651*** (0.294)	-1.492** (0.388)	1.347*** (0.153)	1.335*** (0.149)	1.325*** (0.165)	3.016*** (0.611)
HQ $\times$ Δ Pell Grants (% Tuition)			-0.261 (0.420)	-0.971 (0.826)			0.400** (0.158)	1.528** (0.635)
2-Y $\times$ Δ Pell Grants (% Tuition)				-1.741*** (0.381)				-1.605*** (0.415)
HQ $\times$ 2-Y $\times$ Δ Pell Grants (% T.)				1.416 (0.972)				-1.066* (0.563)
Observations	14,572	14,572	14,572	14,572	63,970	63,970	63,970	63,970
Instrument F-test	92.3	92.4	236.3	296.2	130.5	136.5	106.7	48.4
<i>Panel C: Minority Enrollment Share</i>								
Δ Pell Grants (% Tuition)	-3.779*** (0.445)	-3.782*** (0.445)	-3.878*** (0.482)	-2.473*** (0.645)	1.054*** (0.135)	1.048*** (0.133)	1.035*** (0.137)	2.167*** (0.437)
HQ $\times$ Δ Pell Grants (% Tuition)			0.390 (0.562)	1.009 (1.310)			0.243* (0.136)	1.188*** (0.458)
2-Y $\times$ Δ Pell Grants (% Tuition)				-2.235*** (0.588)				-1.094*** (0.306)
HQ $\times$ 2-Y $\times$ Δ Pell Grants (% T.)				0.093 (1.475)				-0.959** (0.419)
Observations	13,795	13,795	13,795	13,795	57,967	57,967	57,967	57,967
Instrument F-test	77.5	77.2	141.8	223.6	181.7	189.5	159.4	83.1

Notes: School-level 2SLS regressions with school and year fixed effects, standard errors clustered by school in parentheses; *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively. The dependent variable is the two-year growth rate of applications (Panel A), total enrollment (Panel B), and minority enrollment (Panel C), winsorized at 3%/97%. Pell grants (as a share of tuition revenue, winsorized at 1%/99%) are instrumented by the school-level shift-share instrument (for-profit or non-profit version to match each sample). Columns (1)-(4) report for-profit and (5)-(8) non-profit schools: without controls, with controls, adding the interaction with school quality, and adding the interactions with quality and the two-year indicator; controls are lagged tuition reliance for operating revenues, education-related share of expenses, and instruction share of education-related expenses. Quality and 2-Year rows report the interaction coefficients.